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Selection by consequences

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Abstract: Human behavior is the joint product of (i) contingencies of survival responsible for natural selection, and (ii) contingencies of reinforcement responsible for the repertoires of individuals, including (iii) the special contingencies maintained by an evolved social environment. Selection by consequences is a causal mode found only in living things, or in machines made by living things. It was first recognized in natural selection: Reproduction, a first consequence, led to the evolution of cells, organs, and organisms reproducing themselves under increasingly diverse conditions. The behavior functioned well, however, only under conditions similar to those under which it was selected.

Reproduction under a wider range of consequences became possible with the evolution of processes through which organisms acquired behavior appropriate to novel environments. One of these, operant conditioning, is a second kind of selection by consequences: New responses could be strengthened by events which followed them. When the selecting consequences are the same, operant conditioning and natural selection work together redundantly. But because a species which quickly acquires behavior appropriate to an environment has less need for an innate repertoire, operant conditioning could replace as well as supplement the natural selection of behavior.

Social behavior is within easy range of natural selection, because other members are one of the most stable features of the environment of a species. The human species presumably became more social when its vocal musculature came under operant control. Verbal behavior greatly increased the importance of a third kind of selection by consequences, the evolution of social environments or cultures. The effect on the group, and not the reinforcing consequences for individual members, is responsible for the evolution of culture.

Keywords: behaviorism; consequentialism; culture; evolution; law of effect; learning; natural selection; operant conditioning; reinforcement contingencies; social environment; verbal behavior

The history of human behavior, if we may take it to begin with the origin of life on earth, is possibly exceeded in scope only by the history of the universe. Like astronomer and cosmologist, the historian proceeds only by reconstructing what may have happened rather than by reviewing recorded facts. The story presumably began, not with a big bang, but with that extraordinary moment when a molecule came into existence which had the power to reproduce itself. It was then that selection by consequences made its appearance as a causal mode. Reproduction was itself a first consequence, and it led, through natural selection, to the evolution of cells, organs, and organisms which reproduced themselves under increasingly diverse conditions.

What we call behavior evolved as a set of functions furthering the interchange between organism and environment. In a fairly stable world it could be as much a part of the genetic endowment of a species as digestion, respiration, or any other biological function. The involvement with the environment, however, imposed limitations. The behavior functioned well only under conditions fairly similar to those under which it was selected. Reproduction under a much wider range of conditions became possible with the evolution of two processes through which individual organisms acquired behavior appropriate to novel environments. Through respondent (Pavlovian) conditioning, responses prepared in advance by natural selection could come under the control of new stimuli. Through operant conditioning, new responses could be strengthened ("reinforced") by events which immediately followed them.

A second kind of selection

Operant conditioning is a second kind of selection by consequences. It must have evolved in parallel with two other products of the same contingencies of natural selection – a susceptibility to reinforcement by certain kinds of consequences and a supply of behavior less specifically committed to eliciting or releasing stimuli. (Most operants are selected from behavior which has little or no relation to such stimuli.)

When the selecting consequences are the same, operant conditioning and natural selection work together redundantly. For example, the behavior of a duckling in following its mother is apparently the product not only of natural selection (ducklings tend to move in the direction of large moving objects) but also of an evolved susceptibility to reinforcement by proximity to such an object, as Peterson (1960) has shown. The common consequence is that the duckling stays near its mother. (Imprinting is a different process, close to respondent conditioning.)

Since a species which quickly acquires behavior appropriate to a given environment has less need for an innate repertoire, operant conditioning could not only supplement the natural selection of behavior, it could replace it. There were advantages favoring such a change. When

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members of a species eat a certain food simply because eating it has had survival value, the food does not need to be, and presumably is not, a reinforcer. Similarly, when sexual behavior is simply a product of natural selection, sexual contact does not need to be, and presumably is not, a reinforcer. But when, through the evolution of special susceptibilities, food and sexual contact become reinforcing, new forms of behavior can be set up. New ways of gathering, processing, and ultimately cultivating foods and new ways of behaving sexually or of behaving in ways which lead only eventually to sexual reinforcement can be shaped and maintained. The behavior so conditioned is not necessarily adaptive; foods are eaten which are not healthful, and sexual behavior strengthened which is not related to procreation.

Much of the behavior studied by ethologists – courtship, mating, care of the young, intraspecific aggression, defense of territory, and so on – is social. It is within easy range of natural selection because other members of a species are one of the most stable features of the environment of a species. Innate social repertoires are supplemented by imitation. By running when others run, for example, an animal responds to releasing stimuli to which it has not itself been exposed. A different kind of imitation, with a much wider range, results from the fact that contingencies of reinforcement which induce one organism to behave in a given way will often affect another organism when it behaves in the same way. An imitative repertoire which brings the imitator under the control of new contingencies is therefore acquired.

The human species presumably became much more social when its vocal musculature came under operant control. Cries of alarm, mating calls, aggressive threats, and other kinds of vocal behavior can be modified through operant conditioning, but apparently only with respect to the occasions upon which they occur or their rate of occurrence.[1] The ability of the human species to acquire new forms through selection by consequences presumably resulted from the evolution of a special innervation of the vocal musculature, together with a supply of vocal behavior not strongly under the control of stimuli or releasers – the babbling of children from which verbal operants are selected. No new susceptibility to reinforcement was needed because the consequences of verbal behavior are distinguished only by the fact that they are mediated by other people (Skinner, 1957).

The development of environmental control over the vocal musculature greatly extended the help one person receives from others. By behaving verbally people cooperate more successfully in common ventures. By taking advice, heeding warnings, following instructions, and observing rules, they profit from what others have already learned. Ethical practices are strengthened by codifying them in laws, and special techniques of ethical and intellectual self-management are devised and taught. Self-knowledge or awareness emerges when one person asks another such a question as "What are you going to do?" or "Why did you do that?" The invention of the alphabet spread these advantages over great distances and periods of time. They have long been said to give the human species its unique position, although it is possible that what is unique is simply the extension of operant control to the vocal musculature.

A third kind of selection

Verbal behavior greatly increased the importance of a third kind of selection by consequences, the evolution of social environments or cultures. The process presumably begins at the level of the individual. A better way of making a tool, growing food, or teaching a child is reinforced by its consequence – the tool, the food, or a useful helper, respectively. A culture evolves when practices originating in this way contribute to the success of the practicing group in solving its problems. It is the effect on the group, not the reinforcing consequences for individual members, which is responsible for the evolution of the culture.

In summary, then, human behavior is the joint product of (i) the contingencies of survival responsible for the natural selection of the species and (ii) the contingencies of reinforcement responsible for the repertoires acquired by its members, including (iii) the special contingencies maintained by an evolved social environment. (Ultimately, of course, it is all a matter of natural selection, since operant conditioning is an evolved process, of which cultural practices are special applications.)

Similarities and differences

Each of the three levels of variation and selection has its own discipline – the first, biology; the second, psychology; and the third, anthropology. Only the second, operant conditioning, occurs at a speed at which it can be observed from moment to moment. Biologists and anthropologists study the processes through which variations arise and are selected, but they merely reconstruct the evolution of a species or culture. Operant conditioning is selection in progress. It resembles a hundred million years of natural selection or a thousand years of the evolution of a culture compressed into a very short period of time.

The immediacy of operant conditioning has certain practical advantages. For example, when a currently adaptive feature is presumably too complex to have occurred in its present form as a single variation, it is usually explained as the product of a sequence of simpler variations, each with its own survival value. It is standard practice in evolutionary theory to look for such sequences, and anthropologists and historians have reconstructed the stages through which moral and ethical codes, art, music, literature, science, technology, and so on, have presumably evolved. A complex operant, however, can actually be "shaped through successive approximation" by arranging a graded series of contingencies of reinforcement.[2]

A current question at level i has parallels at levels ii and iii. If natural selection is a valid principle, why do many species remain unchanged for thousands or even millions of years? Presumably the answer is either that no variations have occurred or that those which occurred were not selected by the prevailing contingencies. Similar questions may be asked at levels ii and iii. Why do people continue to do things in the same way for many years, and why do groups of people continue to observe old practices for centuries? The answers are presumably the same: Either new variations (new forms of behavior or new

practices) have not appeared or those which have appeared have not been selected by the prevailing contingencies (of reinforcement or of the survival of the group). At all three levels a sudden, possibly extensive, change is explained as due to new variations selected by prevailing contingencies or to new contingencies. Competition with other species, persons, or cultures may or may not be involved. Structural constraints may also play a part at all three levels.

Another issue is the definition or identity of a species, person, or culture. Traits in a species and practices in a culture are transmitted from generation to generation, but reinforced behavior is "transmitted" only in the sense of remaining part of the repertoire of the individual. Where species and cultures are defined by restrictions imposed upon transmission – by genes and chromosomes and, say, geographical isolation, respectively – a problem of definition (or identity) arises at level *ii* only when different contingencies of reinforcement create different repertoires, as selves or persons.

Traditional explanatory schemes

As a causal mode, selection by consequences was discovered very late in the history of science – indeed, less than a century and a half ago – and it is still not fully recognized or understood, especially at levels ii and iii. The facts for which it is responsible have been forced into the causal pattern of classical mechanics, and many of the explanatory schemes elaborated in the process must now be discarded. Some of them have great prestige and are strongly defended at all three levels. Here are four examples:

A prior act of creation. (i) Natural selection replaces a very special creator and is still challenged because it does so. (ii) Operant conditioning provides a similarly controversial account of the ("voluntary") behavior traditionally attributed to a creative mind. (iii) The evolution of a social environment replaces the supposed origin of a culture as a social contract or of social practices as commandments.

Purpose or intention. Only past consequences figure in selection. (i) A particular species does not have eyes in order that its members may see better; it has them because certain members, undergoing variation, were able to see better and hence were more likely to transmit the variation. (ii) The consequences of operant behavior are not what the behavior is now for; they are merely similar to the consequences which have shaped and maintained it. (iii) People do not observe particular practices in order that the group will be more likely to survive; they observe them because groups which induced their members to do so survived and transmitted them.

Certain essences. (i) A molecule which could reproduce itself and evolve into cell, organ, and organism was alive as soon as it came into existence without the help of a vital principle called life. (ii) Operant behavior is shaped and brought under the control of the environment without the intervention of a principle of mind. (To suppose that thought appeared as a variation, like a morphological trait in genetic theory, is to invoke an unnecessarily large *saltum*.) (iii) Social environments generate self-knowledge ("consciousness") and self-management ("reason") without help from a group mind or zeitgeist.

To say this is not to reduce life, mind, and zeitgeist to physics; it is simply to recognize the expendability of essences. The facts are as they have always been. To say that selection by consequences is a causal mode found only in living things is only to say that selection (or the "replication with error" which made it possible) defines "living." (A computer can be programmed to model natural selection, operant conditioning, or the evolution of a culture but only when constructed and programmed by a living thing.) The physical basis of natural selection is now fairly clear; the corresponding basis of operant conditioning, and hence of the evolution of cultures, has yet to be discovered.

Certain definitions of good and value. (i) What is good for the species is whatever promotes the survival of its members until offspring have been born and, possibly, cared for. Good features are said to have survival value. Among them are susceptibilities to reinforcement by many of the things we say taste good, feel good, and so on. (ii) The behavior of a person is good if it is effective under prevailing contingencies of reinforcement. We value such behavior and, indeed, reinforce it by saying "Good!" Behavior toward others is good if it is good for the others in these senses. (iii) What is good for a culture is whatever promotes its ultimate survival, such as holding a group together or transmitting its practices. These are not, of course, traditional definitions; they do not recognize a world of value distinct from a world of fact and, for other reasons to be noted shortly, they are challenged.

Alternatives to selection

An example of the attempt to assimilate selection by consequences to the causality of classical mechanics is the term "selection pressure," which appears to convert selection into something that forces a change. A more serious example is the metaphor of storage. Contingencies of selection necessarily lie in the past; they are not acting when their effect is observed. To provide a current cause it has therefore been assumed that they are stored (usually as "information") and later retrieved. Thus, (i) genes and chromosomes are said to "contain the information" needed by the fertilized egg in order to grow into a mature organism. But a cell does not consult a store of information in order to learn how to change; it changes because of features which are the product of a history of variation and selection, a product which is not well represented by the metaphor of storage. (ii) People are said to store information about contingencies of reinforcement and retrieve it for use on later occasions. But they do not consult copies of earlier contingencies to discover how to behave; they behave in given ways because they have been changed by those contingencies. The contingencies can perhaps be inferred from the changes they have worked, but they are no longer in existence. (iii) A possibly legitimate use of "storage" in the evolution of cultures may be responsible for these mistakes. Parts of the social environment maintained and transmitted by a

group are quite literally stored in documents, artifacts, and other products of that behavior.

Other causal forces serving in lieu of selection have been sought in the structure of a species, person, or culture. Organization is an example. (i) Until recently, most biologists argued that organization distinguished living from nonliving things. (ii) According to Gestalt psychologists and others, both perceptions and acts occur in certain inevitable ways because of their organization. (iii) Many anthropologists and linguists appeal to the organization of cultural and linguistic practices. It is true that all species, persons, and cultures are highly organized, but no principle of organization explains their being so. Both the organization and the effects attributed to it can be traced to the respective contingencies of selection.

Another example is growth. Developmentalism is structuralism with time or age added as an independent variable. (i) There was evidence before Darwin that species had "developed." (ii) Cognitive psychologists have argued that concepts develop in the child in certain fixed orders, and Freud said the same for the psychosexual functions. (iii) Some anthropologists have contended that cultures must evolve through a prescribed series of stages, and Marx said as much in his insistence upon historical determinism. But at all three levels the changes can be explained by the "development" of contingencies of selection. New contingencies of natural selection come within range as a species evolves; new contingencies of reinforcement begin to operate as behavior becomes more complex; and new contingencies of survival are dealt with by increasingly effective cultures.

Selection neglected

The causal force attributed to structure as a surrogate of selection causes trouble when a feature at one level is said to explain a similar feature at another, the historical priority of natural selection usually giving it a special place. Sociobiology offers many examples. Behavior described as the defense of territory may be due to (i) contingencies of survival in the evolution of a species, possibly involving food supplies or breeding practices; (ii) contingencies of reinforcement for the individual, possibly involving a share of the reinforcers available in the territory; or (iii) contingencies maintained by the cultural practices of a group, promoting behavior which contributes to the survival of the group. Similarly, altruistic behavior (i) may evolve through, say, kin selection; (ii) may be shaped and maintained by contingencies of reinforcement arranged by those for whom the behavior works an advantage; or (iii) may be generated by cultures which, for example, induce individuals to suffer or die as heroes or martyrs. The contingencies of selection at the three levels are quite different, and the structural similarity does not attest to a common generative principle.

When a causal force is assigned to structure, selection tends to be neglected. Many issues which arise in morals and ethics can be resolved by specifying the level of selection. What is good for the individual or culture may have bad consequences for the species, as when sexual reinforcement leads to overpopulation or the reinforcing amenities of civilization to the exhaustion of resources; what is good for the species or culture may be bad for the individual, as when practices designed to control procreation or preserve resources restrict individual freedom; and so on. There is nothing inconsistent or contradictory about these uses of "good" or "bad," or about other value judgments, so long as the level of selection is specified.

An initiating agent

The role of selection by consequences has been particularly resisted because there is no place for the initiating agent suggested by classical mechanics. We try to identify such an agent when we say (i) that a species adapts to an environment, rather than that the environment selects the adaptive traits; (ii) that an individual adjusts to a situation, rather than that the situation shapes and maintains adjusted behavior; and (iii) that a group of people solve a problem raised by certain circumstances, rather than that the circumstances select the cultural practices which yield a solution.

The question of an initiating agent is raised in its most acute form by our own place in this history. Darwin and Spencer thought that selection would necessarily lead to perfection, but species, people, and cultures all perish when they cannot cope with rapid change, and our species now appears to be threatened. Must we wait for selection to solve the problems of overpopulation, exhaustion of resources, pollution of the environment, and a nuclear holocaust, or can we take explicit steps to make our future more secure? In the latter case, must we not in some sense transcend selection?

We could be said to intervene in the process of selection when as geneticists we change the characteristics of a species or create new species, or when as governors, employers, or teachers we change the behavior of persons, or when we design new cultural practices; but in none of these ways do we escape from selection by consequences. In the first place, we can work only through variation and selection. At level i we can change genes and chromosomes or contingencies of survival, as in selective breeding. At level ii we can introduce new forms of behavior – for example, by showing or telling people what to do with respect to relevant contingencies – or construct and maintain new selective contingencies. At level iii we can introduce new cultural practices or, rarely, arrange special contingencies of survival – for example, to preserve a traditional practice. But having done these things, we must wait for selection to occur. (There is a special reason why these limitations are significant. It is often said that the human species is now able to control its own genetics, its own behavior, its own destiny, but it does not do so in the sense in which the term control is used in classical mechanics. It does not for the very reason that living things are not machines: selection by consequences makes the difference.) In the second place, we must consider the possibility that our behavior in intervening is itself a product of selection. We tend to regard ourselves as initiating agents only because we know or remember so little about our genetic and environmental histories.

Although we can now predict many of the contingen-

cies of selection to which the human species will probably be exposed at all three levels and can specify behavior that will satisfy many of them, we have failed to establish cultural practices under which much of that behavior is selected and maintained. It is possible that our effort to preserve the role of the individual as an originator is at fault, and that a wider recognition of the role of selection by consequences will make an important difference.

The present scene is not encouraging. Psychology is the discipline of choice at level ii, but few psychologists pay much attention to selection. The existentialists among them are explicitly concerned with the here and now, rather than the past and future. Structuralists and developmentalists tend to neglect selective contingencies in their search for causal principles such as organization or growth. The conviction that contingencies are stored as information is only one of the reasons why the appeal to cognitive functions is not helpful. The three personae of psychoanalytic theory are in many respects close to our three levels of selection; but the id does not adequately represent the enormous contribution of the natural history of the species; the superego, even with the help of the ego ideal, does not adequately represent the contribution of the social environment to language, self-knowledge, and intellectual and ethical self-management; and the ego is a poor likeness of the personal repertoire acquired under the practical contingencies of daily life. The field known as the experimental analysis of behavior has extensively explored selection by consequences, but its conception of human behavior is resisted, and many of its practical applications rejected, precisely because it has no place for a person as an initiating agent. The behavioral sciences at level iii show similar shortcomings. Anthropology is heavily structural, and political scientists and economists usually treat the individual as a free initiating agent. Philosophy and letters offer no promising leads.

A proper recognition of the selective action of the environment means a change in our conception of the origin of behavior which is possibly as extensive as that of the origin of species. So long as we cling to the view that a person is an initiating doer, actor, or causer of behavior, we shall probably continue to neglect the conditions which must be changed if we are to solve our problems (Skinner 1971).

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NOTES

1. The imitative vocal behavior of certain birds may be an exception, but if it has selective consequences comparable with those of cries of alarm or mating calls, they are obscure. The vocal behavior of the parrot is shaped, at best, by a trivial consequence, involving the resemblance between sounds produced and sounds heard.

2. Patterns of innate behavior too complex to have arisen as single variations may have been shaped by geologic changes due to plate tectonics (Skinner 1975).

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Skinner on selection – A case study of intellectual isolation

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Ask yourself the following question: Would "Consequences" have been published in *Science* in 1981 if the author had been anonymous? The answer would be a resounding no, and it would not be difficult to confirm this assertion experimentally now just two years later. Surely the editors of *Science* must have had good reasons for publishing his article. We can only guess the reasons, but I doubt we would be far wrong.

First and foremost, B. F. Skinner is a major figure in modern psychology. Almost anything he has to say in the realm of behavior is of widespread interest whether one's opinion is that it is right or wrong, and with or without adequate documentation. He has made enormous contributions to the field and demonstrated the awesome control the experimenter can have over the behavior of an animal under specified conditions.

That very control seems to have shaped Skinner's perception of the biological world. It has also produced a vision of human behavior that can be disquieting. In "Consequences" Skinner asserts that a person is not "an initiating doer, actor, or causer of behavior." He states further that it is possible to "construct and maintain new selective contingencies" by reinforcing the "good" behavior of such a person. Taken at face value that sounds harmless enough, except for two things: Someone else decides what is good behavior, and we have no clear prescription for how that decision might be reached or who makes it. The definition of good behavior appears simply to evolve by trial and error at three levels, and perhaps it has. That is the major thesis of "Consequences."

The first level is that of Darwinian natural selection. That kind of selection is treated superficially and conventionally. (I return to his views on natural selection below.) The second kind of selection is that of operant conditioning, and the third is that of cultural evolution, the course of both being molded by their consequences. His treatment of the last two levels does not find universal acceptance.

I take exception to Skinner's portrayal of selection at the level of operant conditioning. For one, I prefer to call this level that of phenotypic modification or intraindividual adaptation; the terminology is not important. What is important is that individual adaptability is a much richer set of phenomena than is even remotely embraced by operant conditioning.

Ectothermic animals, for instance, acclimate to the temperature at which they are found; the thermal preferendum of an individual depends on its thermal acclimation, which varies with the season and microhabitat (Hutchinson & Maness 1979). Sexual maturity, with its attendant changes in behavior, can occur at radically different ages in platyfish depending on dominance relationships that are independent of the respondent's behavior (Borowsky 1978). The maternal digger wasp learns how much to provision her nest site in one trial, without the benefit of overt reinforcement, and the appropriate response is delayed several hours (Baerends 1941). Early experience appears to have pervasive effects on behaviors that are first manifest only in adulthood. One such phenomenon is

sexual imprinting; attempts to fit it into a conditioning paradigm present difficulties and suggest a procrustean resolution.

The manner in which Skinner contrasts natural selection and conditioning as two distinct kinds of selection also has a major fault. Genetic and experiential factors are conveyed as being fundamentally separate. This separation is inherent in the way Skinner relegates biologists' interests to unlearned behavior and evolutionary phylogenies. The rigidity is also apparent in his insistence that "most operants are selected from behavior which has little or no relation to" eliciting or releasing stimuli. An epigenetic approach provides a more realistic view.

Evidence is growing rapidly that there are evolved predispositions for animals to learn to respond in particular ways to particular kinds of stimulation. The example most appropriate to Skinner's essay, and also the most debatable, is that of language. People learn a given language, and conditioning doubtless plays a role. But humans may also be predisposed to speak, and the structure of language may have properties that transcend the process of operant conditioning (Lenneberg & Lenneberg 1975). This possibility is ignored in Skinner's essay.

The range of interaction between experience and species-specific constraints on learning is nowhere better and more convincingly documented than in the elegant comparative studies on the acquisition of song among birds (Green & Marler 1979). Likewise, sexual imprinting is proving increasingly to involve both constraints and plasticity; recognizing one's species is an ability that requires little experience. Rather, imprinting's function seems to be the learning of closeness of relationship (P. Bateson 1980). Finally, I disagree with Skinner's easy and almost casual equating of genetic with fixed behavior.

I do agree with Skinner, on the other hand, that cultures have evolved because of the consequences of their practices. Many will differ with us on this.

A major weakness of "Consequences" is that it has been written in a vacuum. Skinner's remarks on natural selection show a lack of understanding as well as total isolation from the noisy arguments that have been heard throughout the land for the last 20 years about group versus individual selection. It is almost embarrassing to read in a 1981 paper that "what is good for the individual or culture may have bad consequences for the species." – or, when writing about the origins of behavior and clearly not about humans, "The behavior so conditioned is not necessarily adaptive; foods are eaten which are not healthful, and sexual behavior strengthened which is not related to procreation." Lest I be misunderstood, let me point out that I am not saying that group selection is inconceivable (see D. S. Wilson 1975; 1980) but that this loose application of the species-benefit argument reveals a fundamental failure to understand modern theorizing about natural selection.

This confusion is apparent in the conclusion of "Consequences." Skinner argues with regard to altruism that selection operates at three different levels, paralleling his opening remarks. The three kinds of selection are (i) biological (here kin selection), (ii) psychological (through reinforcement of individual behavior), and (iii) cultural, (as in inducing heroism). He claims that "the contingencies of selection at the three levels are quite different, and the structural similarity does not attest to a common generative principle."

What we have here is a failure to distinguish between proximate and ultimate mechanisms (E. O. Wilson 1975). The hero is taught to behave that way, which is the proximate mechanism. In the small societies in which heroism must have evolved, the hero's kin enjoyed improved reproductive fitness superior to that of individuals who were not so easy to train. This is kin selection, the ultimate causation. A common generative principle is reasonable.

I was equally taken aback by the absence of references to highly relevant literature closer to home for Skinner. In a classic paper Pringle (1951) explored the parallels between learning and natural selection. Campbell (1975) has written on almost the same theme as Skinner and is often cited. Pringle's and Campbell's treatments are more sophisticated than the essay before us. Skinner has also overlooked the literature on constraints on learning and the ethologists' resolution of the nature–nurture issue.

Why, indeed, should *Science* have published this essay? Perhaps the editors felt that we should know Skinner's mind well, including the box within which it operates, precisely because his writings have been so influential, extending into the sociopolitical arena. Skinner should respond to this type of criticism and not just by asserting that he is not understood. He should acknowledge these other fields by learning more about them than is found in the secondary literature. It may yet be possible to bring this great thinker out of the walls he has erected around his intellect.

On the status of causal modes

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The modern Western world grew up being thoroughly familiar with two traditional causal modes. We knew about the mental mode, the world of ideas and feelings and the associations among ideas and feelings. We knew how to explain behavior in terms of ideation and volition. We also knew about the mechanical mode, the world of machines and billiard balls, projectiles and things in orbit. We knew how to explain behavior in terms of neural impulses and muscle contractions. These modes were familiar; they did not enjoy a great deal of practical success, but they were familiar.

Not so familiar were some other explanatory modes that had been around since antiquity. There was Aristotle's fourfold approach, which showed up from time to time, and which is still fashionable among Jesuit scholars. And there was the empirical approach of Hume. Hume observed, rightly, that we really do not know how the mind works, or even what is really happening on the billiard table. All that we can actually know is that our observations are orderly and, with experience, predictable. If we have observed this following that many times before, then we can count on this following that again. Psychologists have never been very comfortable with Aristotle's approach, nor, for a long time, did they take very kindly to Hume's empirical approach. In the heady days of early behaviorism, the mechanistic bias precluded any alternative to a mechanical mode of causation. All other modes were dismissed, so that behavior could be explained only in terms of neural impulses and muscle contractions.

It is greatly to his credit, I think, that Skinner has always stood opposed to this mechanistic bias. In one of his earliest publications Skinner (1931) considered the question of how one knows that one is looking at a reflex. How can we tell that this regularly elicited reaction really is a reflex? Do we know this because of our underlying knowledge of the neural mediation involved: the afferent, the synapse, and the efferent? No, not at all. We know we have a reflex for simple, empirical, Hume-like reasons: The stimulus regularly elicits the reaction. Back in those days, back at the beginning of Skinner's career, such an empirical orientation was not very popular, but he stuck with it, defended it ably, and continued to promote it. And gradually the empirical causal mode began to catch on.

But meanwhile subtle changes could be seen in Skinner's behavior. He pointed out that just as an elicited response is perfectly well explained by citing the antecedent eliciting stimulus, so an emitted response can be perfectly well explained by citing the consequent reinforcing stimulus (Skinner 1937). Only a mechanist would insist upon the emitted response having an elicitor. Deny the mechanists their fundamental assumption, and you have behavior explained by its consequences. Adhere to a purely empirical mode of causation, and you have operants

ng explained by their reinforcers. If you have a stream of rants, and a stream of reinforcers, these streams turn out to so well correlated that you do not even have to have a eory" to explain the correlation (Skinner 1950). If the stream einforcers really controls the behavior, then you have the iavior under control and you do not have to be bothered by question of how it is mediated, because you have your own d of causal mode.

t may be noted that there is nothing teleological about ıtrol by consequences, because by the time you actually get iavior under control there will be a history of reinforcement it, and if one were interested in questions of mediation then ıt history could be assumed to be acting proactively. But the estion of whether the past acts proactively or the future acts roactively hardly ever arises when one is committed to inner's kind of causal mode.

For some years now (e.g. Campbell 1956) parallels have been ted between the selection of responses by reinforcement and selection of species by evolutionary processes. In "Consequences" Skinner has elaborated some of these parallels. Thus, proposes that causation by consequences is the proper planatory mode for both evolutionary scholarship and the perimental analysis of behavior. And he extends the argument include a third area, social organization and evolution. Perps it was thought that the emphasis upon a common causal ode for these three areas would help to legitimize one or other of them, or the causal mode itself. However, this mphasis overlooks a fundamental point. One can understand at a theorist would want to push an idea as far as it can go, rticularly when it is a good idea. But we also have to underand that parallels may be no more than parallels.

The problem is this. Although it is quite true that evolutiony theorists like to think of consequences (the survival and osperity of a species) selecting genotypes, very little of the plaining they do takes this form. Evolutionary theory is nstantly on the search for mediating mechanisms, for the ıtecedent conditions that present challenges to animals' survival, and to the complicated interactions between animal and vironment. In short, the great bulk of evolutionary scholrship involves itself with proximate causation and mediating echanisms. And the great overriding principle of ultimate usation, the causation by consequences, although very widely lieved in, is rarely cited as explaining anything. In Skinner's alysis of behavior the situation is very much the opposite. He nds to rely on reinforcement as an explanatory principle, and dismiss mediating mechanisms (for example, when there is a scriminative stimulus that might be thought of as eliciting the erant, it is not allowed to do anything, it merely "sets the casion"). So the selection by consequences idea has to carry e whole explanatory burden.

The problem is that different explanatory modes should not mpete with each other, they should complement each other. ristotle was basically right, I think, in that we should use a riety of explanatory modes rather than trying to rely on just e. Perhaps if one is only interested in controlling behavior, en Skinner's causal mode suffices. But a lot of psychologists ant to do more than control behavior. The truth is that the ind is full of ideas and feelings and associations; the head is full neurons and synapses. And a lot of psychologists want to know w these things work, and in their searches they will, no doubt, using the familiar, traditional explanatory modes.

. F. Skinner: A dissident view

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write out of an imperfect and probably biased knowledge of inner's work, my reading not extending much beyond his popular works and the present article, "Consequences." It is only by the clear statement of current images and subsequent testing by critique or experience, however, that knowledge becomes more perfect. The following is my current image, subject to future revision.

1. *I still have to be persuaded that experimental psychology*, especially with animals, has contributed much even to the understanding of animals, still less to the understanding of the immense complexity of the human organism. The experimental method is useful only in a very limited area of scientific inquiry, where systems are simple and repeatable, as in chemistry. It is on the whole an inappropriate methodology in developing improved cognitive images of complex, unstable systems with changing parameters and cumulative structures, where rare events are significant. Humans are a supreme example of systems of this kind.

2. The whole black-box, input–output, behavioral approach strikes me as having very limited value in understanding behavior, even of animals. In the case of humans we have a key to opening the black box of our minds in our capacity for reflection and communication. It seems the height of absurdity to dismiss this as "operant control" of "vocal musculature."

3. Skinner's basic theoretical concepts, such as reinforcement, rest ultimately on a largely unexamined selection of mental images out of a potentially very large and unexamined repertoire. It simply assumes that there is a valuation structure within the organism which, for instance, can distinguish, and at least rank, pleasures and pains, and this assumption presumably is derived from human introspection and then applied to rats and pigeons. Valuations, however, although they are in part genetically created, are also, even in animals, learned, and in humans the learning process is very large. The evaluative structure, for instance, by which we learn to speak and write "correct" English is wholly learned – we have a genetically produced potential for the learning of language, but there is no gene for English or Chinese.

4. The evolutionary theory, as expounded in "Consequences," is a middling first approximation, but quite inadequate even for the complexities of biological, much less for social, evolution. It neglects the complexity of ecological selection, and even more the complexity especially of societal mutation, which is often highly teleological and much influenced by the capacity of humans for images of the future. Together with the sociobiologists, Skinner neglects the process of transmission of learned structures from one generation to the next by a learning process, which I have called "noogenetics," and which is of some importance even in animals, and of overwhelming importance in humans. Skinner does recognize, however, that selective processes that lead to the spread of a particular mutation (and this goes for the noogenetic as well as the biogenetic) through the field of a species may be adverse to the survival of the species itself. The reverse may also be true – that obstacles to the spread of certain mutations through the species may help the survival of the species itself.

5. With Skinner's protest against crude applications of classical mechanics I have a lot of sympathy, the very success of Newtonian and Laplacian mechanics has had a most unfortunate effect on those sciences that study complex systems with unstable parameters, where whatever "laws" there are change all the time. He constantly seems to be slipping back, however, into the quasi-mechanical determinism the theory of operant conditioning represents, which is inappropriate to biological and especially to human and societal systems. Skinner, and many other psychologists, have trapped themselves in a methodology inappropriate to the system they are studying, so much so that it is hard not to feel that we can learn more about human structure and process from the poets than from the psychologists. It could be, of course, that when and if we ever find out the actual processes in the nervous system by which images are coded and changed, this could lead to a radical change in the understand-

ıg and practice of human learning. This, however, requires bservational and descriptive science, guided by fine instruıentation, rather than experimental science. Here again the rinciple that nothing fails like imitated success seems to apply; he success of experimental science in its appropriate fields, vhich are quite limited, has led to its extension into fields in *vhich it is not appropriate.* The psychologists, and Skinner in articular, seem to have been caught in this trap.

Behaviorism and natural selection

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Much of the content of "Consequences" has appeared preiously in other publications of Professor Skinner. I have taken his opportunity to read some of them also. It is interesting to ee so much emphasis on evolution and natural selection in the vork of a distinguished experimental psychologist. As he himself points out, few psychologists afford selection much attenion. Surely one of his many contributions has been the stressing f selection and the role of behavior in it. Nevertheless, there re some elements in the paper with which I disagree.[1]

Although Skinner inserts the disclaimer that his three levels f contingencies are ultimately all a matter of natural selection, hroughout the bulk of his discussion here and elsewhere he reats them quite separately. Further, he emphasizes their listinctness by his statement that each of the three levels of ariation and selection has its own scientific discipline: biology, psychology, and anthropology. He suggests that operant conditioning, the second level or kind of selection by consequences, ould supplement natural selection or replace it. It is my ontention that subsuming these three entities – natural selection, operant conditioning, and social environment or culture – nder the umbrella of "selection by consequences" is misleading.

The modern definition of natural selection stresses reproductive success. The genotypes of a species possess a large pool of ariation. This produces phenotypic variation in the populations f the species, some of which is morphological and some behavioral. Natural selection is a statistical concept. The better genotype has a better chance of surviving long enough to reproduce itself and add some of its genes to the next generation. It should e remembered that natural selection favors or discriminates gainst phenotypes, not genotypes. When genotypic differences are not expressed in the phenotype they are not ccessible to selection. This process is said to be selective ecause some genes increase in frequency while others ecrease.

Operant conditioning or contingencies of reinforcement may e said to select for increased or decreased frequency of some orm of behavior in the repertoire of individuals. Such behaviors ight become shaped in such a way as to be appropriate for ovel environments and lead to differential reproduction. For his to occur these behaviors or their possessors would have to e acted upon by natural selection. Operant conditioning is here cting much like mutation or gene recombination, producing ew variation on which natural selection may act. It does not ave a power equivalent to that of natural selection insofar as volution is concerned. The evolution of social environments or ultures is perhaps analogous to biological evolution. Certainly t has had an impact on human evolution. In my view, classifying atural selection, operant conditioning, and the evolution of ulture as three "kinds" of selection by consequences is akin to lassifying a child's ball, the planet earth, and an orange as pheroids.

In "Consequences" Skinner again deplores the use of the erm "selection pressure" because he considers it to be an example of an attempt to assimilate selection by consequences to the causality of classical mechanics. It appears to convert selection into something that forces a change. In his book *About Behaviorism* (1974) he raises the same complaint and adds that the notion that the "pressure" is exerted primarily by other species is erroneous. The definition of selection pressure with which I am familiar is: the degree of systematic bias or enhanced probability in favor of increase, from one generation to the next, of the frequency of a given genetic factor or type of genetic system (Jepsen, Simpson & Mayr 1949). I do not see how this concept could be considered to be an attempt to assimilate selection to the causality of classical mechanics. Perhaps the term "pressure" is too reminiscent of physics, but the actual definition gives no such impression. Skinner does not reveal the origin of the idea that selection pressure is exerted primarily by other species. I do not believe that such an idea is common among biologists.

Skinner clearly believes that the concept of selection by consequences is an explanatory scheme or causal mode with more explanatory power and verifiability than some of the more traditional schemes. As he indicates, natural selection occupies a very special place. Although Darwin was able to persuade many that evolution had indeed occurred, he had more difficulty convincing them that natural selection was and is the creative force of evolution. Certainly it can be said that among biologists its validity is reasonably well established now. The punctuationalists are, however, currently contending with the gradualists concerning the temporal relationships of its actions (Eldredge & Gould 1972). The evidence for social and cultural change brought about by changing environments is all [illegible] These changes result less from alterations in the [illegible]vironment than from human activity. I am not c[illegible]t that a strict analogy with natural selection is possible i[illegible] instances. My value system tells me that many of these chang[illegible] are deleterious. Operant conditioning is certainly a ver[illegible]ful tool for understanding and in many cases controlling behavior. Where it is appropriately applied it is vastly more useful than the invention of forces, essences, and deities.

NOTE

1. This material has been reviewed by the Walter Reed Army Institute of Research, and there is no objection to its presentation or publication. The opinions or assertions contained therein are the private views of the author and are not to be construed as official or as reflecting the views of the Department of the Army or the Department of Defense.

Skinner, selection, and self-control

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Recognizing that Skinner wants us to accept the metaphor of "natural selection" as the exclusive metaphor for theorizing in psychology, we can simply retort that we find this metaphor not helpful enough, that we find his arguments and facts supporting the role he wants to give this metaphor not convincing enough. Outstanding examples of this line of criticism are Chomsky (1959) and Dennett (1978b).

The problem with this criticism is that however convincing it is to most of us, Skinner himself does not seem to give it much weight. His view seems to be that whatever critics profess to show along this line, it is still the case that his choice of metaphor is superior and exclusive. But if Skinner's reason for his choice of metaphor is not its proven value, what could his reasons possibly be? Well, one might think that Skinner does not really think of "selection by consequences" as a metaphor at all. There are indications of this in "Consequences." Current theorizing in cognitive science is characterized as operating with "the metaphor of storage," but selection by consequences is said to be

recognized in natural selection" (my italics) and "*dis-d* very late in the history of science" (my italics). But there l be no doubt that the idea of natural selection is a horical extension of a notion used to characterize the n practice of animal and plant breeding ("artificial selec-. In some of Darwin's forerunners (see the discussion of and Matthew in Mayr 1982) this was explicitly indicated. in himself was immensely happy with this analogy, and it d his language to the extent of describing nature as a ing agent on a par with a human being. He viewed icial selection" as an experimental verification of his theo-*t more important, I think, is the vestige of* "goal directed-assigned the process of evolution by this metaphor.

e obvious problem we still have in understanding Darwin's y is to a large extent attributable to difficulties in sorting hat should be taken literally and what should be seen as metaphorical in this notion of "selection." What does it to say that "nature selects"? In what sense is the process of ation a matter of "selection"? In view of this it is unreason-to treat "selection by consequences" as anything but a phor. This is not to degrade it: Theorizing thrives on good phors. But it leaves us with a substantial problem of sis: What does "selection by consequences" mean? And is *metaphor* good enough? To say that "selection by conse-ices" was recognized or discovered may seem harmless gh, but it can convey a seriously misleading impression. ow, I don't think that Skinner really wants to convey this ression, and this is indicated by claims like the following, n he is comparing the "prescientific" view of persons with wn (Skinner 1971, p. 101): "Neither view can be proved, it is in the nature of scientific inquiry that the evidence ld shift in favor of the second."

us, another reason for Skinner's confidence might be found the nature of scientific inquiry." Skinner derives his views sychology and anthropology from the field of biology. And eems to be taking the development within biology as an cation of what direction "scientific inquiry" will take. Just as ogy, with the aid of "natural selection," has done away with a or act of creation," "purpose or intention," "certain es-es," and "certain definitions of good and value," according kinner, so will psychology and anthropology as they mature. brings us to a second line of criticism of Skinner's position. his line is more interesting if one really wants to disturb mer, something the first line obviously hasn't been able to Let us then tentatively accept Skinner's claim that "selec-by consequences" is the supreme and exclusive metaphor heorizing in psychology. Is Skinner's understanding of this aphor, his understanding of Darwin's theory of natural ction, accurate? Specifically, does the acceptance of this aphor, the extension of Darwin's theory into psychology, mit one to a view of persons as not "initiating agents" usal forces"? [See also Dennett: "Intentional Systems in nitive Ethology" *BBS* 6(3) 1983.] There are at least five ects of Darwin's theory that point to negative answers to e questions. (Dennett 1978c, which arrives at a similar clusion using different arguments, is an admirable example is line of criticism.)

Darwin's theory of evolution has two major components: a ciple of variation and a principle of selection. Skinner is enced by the synthetic theory of evolution and its simplistic of the first of these principles. But once the processes of ation are taken seriously, and recent trends in evolutionary ory indicate that they again will be, the organism itself mes an important object of study. This means, among other gs, an interest in embryology and morphology (epigenetics). nsequence of this interest is a questioning of the hegemony laptationism in favor of "structural" considerations (Gould & ontin 1979, Grene 1959).

Obviously, natural selection works only if organisms are tal. Only if organisms are threatened by destruction and failure to procreate will there be any "consequences" to consider. This was the very important truth that Darwin learned from Malthus. His theory of evolution rested squarely on the premise of there being a struggle for survival. But this necessitates an interest in the characteristics of organisms determining their conditions of survival.

3. Natural selection favors organisms with adaptive traits. Traits, however, are not adaptive as such, but only relative to other traits of the organism. What good will the long neck do the giraffe if its teeth are not strong enough to chew the leaves, or its stomach not strong enough to digest them? Selection by consequences is not a process determined by the environment alone; it is the combination of organism and environment that does the selecting.

4. Organisms with their behavioral repertoires are selected by their environment. But there is an opposite process of selection by which organisms select their environment. This is perhaps a more important process of adaptation in cultural species. Thus, organisms adapt by changing and by changing their environment. Indeed, the latter process is central to Skinner's program of education and social reform. Just as we can learn about a well-adapted organism by studying its environment, so we can learn about its environment by studying the organism.

5. Romantic Europeans in the early 19th century dreamed of the liberation of nations, the usurpation of the power of despots, of self-rule and democracy. Darwin's theory of evolution usurped a "very special creator," to use Skinner's phrase. But Skinner doesn't see that this liberation of nature involves a transfer from outside control to self-control. The very essence of Darwin's "Copernican revolution" is, I think, the idea of nature as a self-regulating, self-controlling system. But if control can be transferred from a creator to the system of nature, it can be delegated from nature to its subsystems.

A first step toward the liberation of nature is made possible by viewing nature as a machine, which once started will run by itself. This mechanistic view was central to 18th-century deism. The idea is that a rigid universe needs no controller. But a flexible, evolving nature must be controlled to be protected from destruction. Nature controls its own flexibility by constraining it – "Natura non facit saltum" – and its consequences – through immediate, constant, and relentless selection – according to Darwin.

In an evolving nature there is flexibility, variation, in the reproduction of organisms. Darwin's theory explains how such a flexible reproductive system can survive through self-control. But for the system to survive its organisms must survive long enough to reproduce. This, again, can be ensured by the organisms being rigid, like machines. But if an organism is flexible it must be protected. Darwin was struck by the enormous waste in nature, by the fierceness of the "struggle for existence." Nature is very different from the benign environment of the Skinner box. It must therefore be possible for a flexible organism to avoid self-destructive behavior by self-control, rather than by environmental control, since the latter, in a harsh environment, is equal to destruction. Responses must be killed off without killing the organism. If the organism can foresee the consequences of its behavior, it can avoid self-destructive behavior. Selection must be delegated from the environment to the organism itself. Rather than taking a step over a cliff and dying, a human being can let the step die instead. In this way, nature can admit flexibility in its subsystems by delegating control. Organisms too, must be liberated.

As this liberation of subsystems proceeds systems will appear with goals that, temporarily at least, may conflict with the goals of the total system. From the Darwinian perspective it is clear that the behavioral capacity of a human being is part of a functional system in which the goals themselves are flexible, and only indirectly related to survival and procreation. But even to the extent that the goals of human beings can be related to goals

ction misconstrued

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ıer makes at least three claims that attract the attention of olutionary biologist: (a) that conditioning works on indi-l behavior in a manner strictly analogous to the operation ural selection in evolution; (b) that this analogy sheds light e evolution of learning and on the impact of learning on ıtion; and (c) that more attention needs to be paid to the ss of selection and less to the objects selected. How far do insights take us, and are they new?

wontin (1970) stated quite clearly that as long as things vary heritable manner and undergo differential survival and oduction, those things will evolve, whether they be orga-s, ideas, or cultures. Dawkins (1976) has taken the conse-ces of that idea much farther, and it is now a familiar, if not ersally accepted, part of evolutionary biology. Thus Skin-first claim is not new.

question his repeated presentation of natural selection as ming traits for the benefit of species. Skinner seems to be vare of the controversy over group selection, individual tion, and gene selection that has occupied the attention of utionary biologists for the last 20 years. It has considerably fied the way we think about natural selection (see Maynard h 1964; Price 1972; Wade 1978; Williams 1966). Although lebate is not over, it seems safe to say that only under special umstances could one conclude that a trait had evolved for benefit of the species. In nearly all cases, traits have evolved use they increased the fitness of genes, individual organ-, or both. There is some persuasive evidence that certain avioral traits have evolved because they increased the in-ive fitness of individuals, that is, fitness gained through ives (Hamilton 1972; Sherman 1977), and some evidence cates that certain traits, such as female-biased sex ratios well 1981; Hamilton 1967) and recombination itself (May-l Smith 1978), have evolved in part through group selection. vever, biologists continue to produce alternative explana-s based on individual selection or gene selection (e.g. enstein 1974; Hamilton 1980; Rice 1983) for even those s, such as recombination, for which the evidence is most istent with group selection. Skinner also seriously under-nates the rapidity of evolutionary change. Significant nges can occur in ecological time, on the order of tens of erations (Kettlewell 1961; Stearns 1983). Skinner's view of ural selection is badly out of date.

inner's most constructive point is to call attention to the raction of selection and learning. I particularly appreciated lescription of how the evolution of learning could lead to adaptive or even to maladaptive behavior. However, he n falls disappointingly short of the sort of analysis that would uggested by recent developments in evolutionary biology. rning is a sophisticated form of developmental plasticity. by (1956) has suggested that plasticity in some traits may er other traits against the force of selection, and Caswell 3) has developed this idea in the context of the evolution of oductive traits. Charnov and Bull (1977) have argued that e sex determination is favored by natural selection when an vidual's fitness – as male or female – is strongly influenced nvironmental conditions and when the individual has little rol over which environment it will experience. Stearns and ıdall (1983) have extended this argument to traits and ronments that can vary continuously. These analyses of behavioral traits provide a prototype for a biologically based ysis of the evolution of learning. Cavalli-Sforza and Feldman 1) have recently published an extensive discussion of the raction between genetic and cultural evolution. In all these ogical discussions there are many more concrete points for urture and suggestive hypotheses about the evolution of learning and the impact of learning on evolution than are suggested by Skinner's straightforward analogy. What things are easy to learn, what things are hard to learn, and why did that distinction evolve? Skinner passes that essential question by.

Skinner's final notable point is that selection processes are blind mechanisms, and that too much attention has been paid to the object selected and not enough to the process of selection. I completely agree with his denial that organisms can be construed as initiating the process of adaptation, and with his plea that more attention be paid to the process of selection, but I cannot countenance his decision to treat the organism as a black box and to concentrate solely on the external circumstances affecting the organism. We must understand enough of the internal structure of organisms to explain how they interact with their external circumstances. That internal structure can make a critical difference to the predictions one would make both about the direction of evolution and about the development of behavior.

In summary, Skinner's analogy between evolution and learning is apt but hardly new, and his picture of evolution errs in its details. The questions Skinner states explicitly do not seem likely to lead to productive new lines of research, but the questions he states implicitly are loaded with significance: How does learning evolve? Once evolved, what implications does learning have for subsequent evolution? Does it uncouple phenotype and genotype, as suggested by Huxley (1942)? What things are hard to learn and what things are easy to learn? Biology and psychology can interact productively in these areas (see Kamil & Sargent 1981). Skinner enters a final plea for an objective assessment of the causes of human behavior. Although I do not think his scheme of selection by consequences is adequate to represent causation, I do agree that we must understand the biological basis of human behavior and the limits it sets – if any – on what can be learned. That knowledge would make it easier to solve problems caused by our own behavior.

Selection by consequences: A universal causal mode?

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For some time it has been customary to attribute human behavior to a combination of the effects of biology, individual learning, and society. Though it is clear that these determinants are related, experience has taught most of us to consider them separately rather than to ignore divisions between disciplines and suffer the cross-fire of individuals defending their turf. Skinner has frequently crossed such disciplinary boundaries, in the present case by arguing that selection by consequences is the key causal mechanism in all three areas. Though I admire his bravery and have sympathy for his goal of integration, I think the resemblances Skinner sees are overstated and his emphasis on a common causal mechanism interferes with rather than facilitates analysis of the relations among natural selection, individual learning, and culture.

What Skinner calls attention to is that changes in evolution (new species), individual learning (new behaviors), and culture (new societies) typically occur in the context of some alteration of the environment. On the basis of this resemblance Skinner argues that the changes are based on the common causal mechanism of selection by consequences. The fundamental problem with this argument is that a consequence is by definition an outcome that follows necessarily from a set of conditions. By this definition only operant learning can be a product of selection by consequences; examples of natural selection and cultural evolution do not follow from any set of prior conditions that can be specified. I develop and add to this argument below. Since

Skinner has insightfully argued), and if selection-by-consequence mechanisms provide the modern explanation for intelligence and purpose (as Skinner affirms), and if such mechanisms need not be seen as denying the existence of the phenomena they explain (as many modern mechanistic mentalists maintain), then on what grounds do we dismiss the possibility that evolving species are not in some sense individuals (Ghiselin 1981) of limited but genuine intelligence and purpose (Schull, in preparation)? The very question is enough to drive one back to behaviorism.

Bridges from behaviorism to biopsychology

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It would be difficult to overstate the magnitude of Skinner's contribution to psychology and related disciplines. Many of the approaches to the study of behavior that we take for granted can be traced to his initiative. Yet Skinner has not been without his critics. On the one hand there are those who express moral or philosophical outrage at some of his ideas about the control of behavior. At a more concrete level, there are those who have criticized specific aspects of Skinner's views. Some of the most severe criticisms have come from biopsychologists and neuroscientists. Skinner has often been criticized for his treatment of the biological aspects of behavior. This is especially true concerning his treatment of language and the predispositions for learning, and his general views on the brain. And although some of the tensions between radical behaviorism, biopsychology, and neuroscience will not be easily resolved, in "Consequences," Skinner appears to be building some bridges.

One of the most stinging criticisms of Skinner's work concerns his view on language. The major thrust of this criticism is that Skinner has ignored the biological aspects of language. A second point of contention between Skinner and his critics concerns the apparent incompatibility between Skinner's views on the acquisition of operantly conditioned responses and the work showing that certain associations are more easily learned than others (e.g. Garcia, McGowan & Green 1972). Here Skinner has been criticized for ignoring biological predispositions which either facilitate or hinder learning. A third criticism leveled by biopsychologists and neuroscientists concerns Skinner's treatment of the brain. To some, Skinner's assertion that it is not useful to study and model the nervous system through inference from behavior ignores many of the insights obtained about brain–behavior relationships using just this strategy.

Skinner has not ignored his critics. In "Consequences" he both revitalizes earlier arguments and marshals new ones to answer these criticisms. Yet the tone of this article seems more compromising than that of earlier treatments. In discussing language, Skinner seems to have modified his earlier views somewhat. In considering the importance of language he notes that "vocal behavior can be modified through operant conditioning, but apparently only with respect to the occasions upon which [the various kinds of vocal behavior] occur or their rate of occurrence." With this statement, it appears that Skinner has made room for the biological aspects of language, while at the same time providing an important role for operant conditioning.

In "Consequences" Skinner also attempts to accommodate the compelling data concerning biological predispositions for learning. For example, he notes that a duck may follow a large moving object (in this case its mother) both because it has been selected to do so and because of "an evolved susceptibility to reinforcement by proximity to such an object." This is a far cry from Skinner's earlier statement:

> Pigeon, rat, monkey, which is which? It doesn't matter. Of course, these species have behavioral repertoires which are as different as their anatomies. But once you have allowed for differences in the ways in which they make contact with the environment, and in the ways in which they act upon the environment, what remains of their behavior shows astonishingly similar properties. (Skinner 1959)

In addressing language and biological predispositions for learning Skinner appears to have accommodated the concerns of many biopsychologists. But what about Skinner's treatment of the brain? Many have accused Skinner of treating the brain as a black box and simply ignoring it. This, of course, is not true. Rather, Skinner has asserted that the brain should be studied in much the same way that all other events are studied – by examining observables. Skinner therefore soundly rejects the notion of the "conceptual nervous system." He argues that a conceptual nervous system cannot be used to explain the behavior from which it is inferred (Skinner 1974, p. 213). Many behavioral neuroscientists would disagree with this statement. The conceptual nervous systems generated to help explain sensory (Helmholtz 1852), motor (Sherrington 1947), and associative (Pavlov 1927) processes were all inferred from behavior, and each has served a valuable heuristic role in subsequent work on the neural aspects of behavior. To deny the value of such models is to dismiss the basis for substantial progress in the neurosciences.

Skinner also rejects the notion that behavior can be completely understood by understanding the brain and nervous system. Instead, he has argued that the goal of neuroscience (he calls it the promise of physiology) is to describe how the nervous system mediates the contingencies between a discriminative stimulus, a response, and a reinforcer. In doing this, Skinner has argued that it is behavior that neuroscientists must strive to explain. It is this adherence to the notion that behavior is the phenomenon that neuroscientists must consider focal that forms Skinner's most valuable contribution to the study of the brain. Through this argument he has suggested that a theory of brain function can be meaningful only if it is posited in the context of behavior.

Consider, for example, how neuroscientists study memory. One approach is to understand the anatomy, physiology, and pharmacology of the nervous system with the hope of eventually understanding how various neuronal processes combine to code memory. The goal here is to uncover mechanisms of plasticity that *might* be responsible for coding memory (see, for example, Swanson, Teyler & Thompson 1982). As those working in this field recognize, much of the work using this approach is both elegant and important. Yet because it divorces itself from behavior, I doubt that Skinner would favor it. Skinner's approach is better reflected in the model systems approach to studying neural mechanisms of learning and memory (see Kandel 1976; Thompson 1976). Here the beginning point, the incontrovertible evidence which must always be considered, is the behavior of the organism. All attempts to understand possible mechanisms of memory must stem from and be consistent with a particular learned behavior. This seemingly simple point that many of us studying brain–behavior relationships now take for granted has its foundations in the work of Skinner and other behaviorists.

In "Selection by Consequences" Skinner has built bridges between the often separated approaches of behaviorists and biopsychologists. This is especially true in his treatment of language and biological constraints. There still appears to be some tension between radical behaviorism and the neuroscience community concerning the relationship between brain and behavior. Yet behavioral neuroscientists in particular should take note of Skinner's basic contention: Behavior is the starting point for studying brain function, and any final understanding of brain function must be commensurate with the behavior it is evoked to explain.

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spective of behavior to be incorporated into the fabric of ld societies it will have to be coupled with a social philoso- , not yet in hand, that will provide or define a "purpose" for tence for the individual.

Whatever our individual perspectives regarding behavior and future, we have been very fortunate to have the brilliant tributions of B. F. Skinner. His contributions to psychology serve in perpetuity as part of those contingencies that will tinue to bring it to new states across time. It has been a sonal privilege to be challenged by his life and perspective ough the 30 years of my own career.

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lectionism, mentalisms, and behaviorism

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long as we cling to the view that a person is an initiating r, actor, or causer of behavior, we shall probably continue to lect the conditions which must be changed if we are to solve problems." No wonder Skinner finds the "present scene" couraging. Species *do* adapt to an environment *and* the ironment selects adaptive traits; individuals patently *do* ust to situations *and* situations maintain and shape adjusted avior; groups of people *do* solve problems, *even as* circum- nces do select cultural practices that (sometimes) yield solu- ns. The problem with Skinner is that he has always insisted t recognition of the importance of selection would require ndoning any appreciation of purposes or persons as causal nts. The problem with the present scene is that it has taken n at his word and chosen dignity, purpose, and the acknowl- gment of cognition over behaviorism and selectionism.

The cultural practice we should all consider abandoning is the mise that selectionism necessarily implies behaviorism. That inner would resist such a move is not surprising, given his tory of reinforcement. His position was shaped at a time en cogent arguments against a scientifically sterile and meta- ysically dubious mentalism were amply rewarded, and with od reason. But today, disciplined analyses of cognitive func- ns in psychology, biology, and cognitive science are among most fertile approaches, and they are implicitly or explicitly nded upon the rejection of the vitalistic mentalism that so ensed behaviorists. Perhaps the time has come to try to derstand the cultural context that selected for behaviorism, to derstand why selection by consequences remains an under- preciated causal mode, and to understand how the cultural d probably innate) tendency to impute purpose to biological enomena can persist in a world that is (undeniably and ially) produced through selection by consequences occur- g at many levels.

In the context of a Cartesian dualism which said that phe- mena were caused *either* by mindless, mechanical, material, d scientifically explicable processes *or* by nonphysical, scien- cally inexplicable mentalities, William Paley's (1836) pre- rwinian review of the otherwise inexplicable fitness of organ- s to their environments was justifiably taken to provide ong evidence for the existence of an intelligent, purposive, *ine* "designer" of the biological world. Paley argued for scartes' god. The theory of "natural selection replaces a very cial creator" precisely because it explained adaptedness in ms of purely material processes. The mind of god was there- e expunged from biology. Skinner's and Thorndike's (1911) n advancement of the physically realizable principle of rein- cement was similarly taken to be incompatible with the idea that mental processes underlie voluntary behavior. But if we abandon the idea that physical realizability precludes mentality then we are free to get on with the business of using selectionist principles to explain behavior *and* cognition in initiating agents, without binding ourselves to the facts of life and mind.

This is not to say that any of us initiating agents are unmoved movers, only that we, and our feelings of causal efficacy, are worth attending to. An example which Skinner ought to accept might help us make this point. The initiating conditions of the *evolution* of new behavior patterns are woefully neglected in biology because biologists tended to shy away from phenomena in which the achievements of individuals could instigate evolutionary change. Such phenomena are suggestive of purposivism, Lamarckism, and other bugaboos, and have therefore been eschewed. However, one of the few recent discussions of a probably important phenomenon of this sort comes from none other than Skinner himself. In a note on the phylogeny and ontogeny of behavior Skinner (1969) [see "Phylogeny," this volume] supposes that an ancestor of the modern dog had no instinctive tendency to turn around as it lay down, but learned the behavior as an operant reinforced by the production of a more comfortable bed. Once instigated, however, the behavior might have had adaptive consequences (permitting quicker movement in emergencies, etc.) which could select for genes that promote the behavior. If so, "Dogs in which the response was most readily conditioned must have been most likely to survive and breed," and the behavior may have "eventually become so readily available as an operant that it eventually appeared without reinforcement" (Skinner 1969, p. 204). Evolutionarily speaking, that first creative individual was an initiating agent even though its behavior (like all events) had causal and selective antecedents. The example suggests (1) that species with some developmental plasticity may be able to "experiment" with potentially adaptive traits for many generations before they become "committed" to the relatively "expensive," slow, and hard-to-reverse process of genetic institutionalization of the trait; (2) that recognition of initiating agents need not entail neglect of selection by consequences; and (3) that interactions between the kinds of selection Skinner mentions bear further discussion (Baldwin 1896; G. Bateson 1963).

In a similar manner, intelligent agents (like ourselves, for example) "experiment" "mentally" with potentially productive courses of action (rehearsing various scenarios, envisioning their probable consequences, and selecting the one that has produced the most desirable imagined consequence) before committing themselves to the relatively expensive, slow, and hard-to-reverse process of behaving. If the imagined consequence is in fact experienced, their "foresight" is rewarded – which is just to suggest (with James 1890, Dennett 1978, and others) (1) that thought processes themselves involve selection by consequences, and are selected by mental and environmental consequences; (2) that the parallel between selection in individuals, in species, and in cultures goes deeper than Skinner permits himself to imagine; and (3) that imputing a causal role to thought, purpose, and goals does not force one to abandon the ideal of physiological realizability (or physical determinism, for that matter). The investigation and exploitation of the neural and environmental process underlying such phenomena would proceed more rapidly if Skinner's habitual linkage of selectionism with behaviorism and antimentalisms were rethought. Selectionism does not preclude mentalism, and I believe Skinner has made it clear that mentalism requires selectionism. [See also Dennett: "Intentional Systems in Cognitive Ethology" *BBS* 6(3) 1983.]

On the other hand, it should be noted that if a materialistic cognitivism is permissible in contemporary psychology, it may not be excludable from evolutionary biology. The parallels between evolution, learning, and thinking in species and in individuals serve to make another point. If species are hierarchically organized selection-by-consequence systems (as

intermediaries. On the other hand, if the only other source for manageable classes of identifiable intermediaries (the common effects diverse reinforcers share, which enable us to explain and predict the behavioral consequences of reinforcement) are to be found in neuroscience, then the entrenchment and further development of the law of effect will have to be postponed, pending the establishment of a neuroscience with the desired manageable classes. The discovery of biochemical mechanisms of synaptic transmitter modulation by classical conditioning (reported in Kandel & Schwartz 1982) may justify some optimism in this regard, but what it shows is that the elaboration of a psychological theory of selection by consequences must, like its evolutionary big brother, proceed in the direction of a molecular biological, that is physical, theory.

Of course, there may be intermediaries of the required sorts at higher levels of organization than those treated in molecular neuroscience. This is the hypothesis of the cognitive psychologist, among others. There is, moreover, some reason to suppose that if such intermediaries exist, their behavior is shaped by selection for consequences as well (see Dennett 1978a). If so, Skinner will have been vindicated in spite of himself, for the fundamental units of an operant psychology will be the kind of ghostly intermediaries he has so long condemned. But given the parallels between operant psychology and evolutionary biology, this should come as no surprise, for just as the units of selection and evolution are in dispute among biologists (see Ghiselin 1981), we should expect that in the parallel science of psychology, the units of selection may not, as Skinner blithely supposes, be "selves or persons." In either case, the future direction of development for a psychology of selection by consequences must be away from behavior and its environmental conditions, in the very direction Skinner has so sternly enjoined.

Perspectives by consequences

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Skinner argues persuasively that behavior is selected by consequences and that contingencies of reinforcement shape operant responses. Consistent with this principle is the conclusion that perspectives on behavior held by scientists are themselves formulated or shaped by contingencies of reinforcement, by consequences. Skinner himself recognizes this congruence and has on several occasions denied that he is to receive any personal credit for his contributions to science: Those contributions have been beyond his personal control in that they have been shaped and articulated by the same set of principles that he attempts to define as sufficient for the entire behavioral domain.

It is concluded that the formulation of the laws of behavior by scientists will be dictated by those same laws; however, only if validity is reinforcing will their articulation eventually be errorless and complete. By the same reasoning, those laws and principles will also serve to shape acceptance, or rejection, by the scientific community and by the public at large of the laws and principles as articulated. It is to be hoped that the responses of acceptance by scientists will become increasingly enthusiastic as the articulation of the principles or laws becomes increasingly sufficient. But will this be the case? If so, will it also be the case that societies at large will respond with increased acceptance as scientists more closely approximate the translating of natural law into the language of humans?

Skinner argues in "Consequences" that society is selected for and shaped by "whatever promotes its ultimate survival." Each society has had its own set of characteristics shaped by contingencies spanning millions of years, and no radically new characteristic can accrue to a society as though it were a biologic mutation. Consequently, it is improbable that we can anticipate or define for an array of diverse societies all of the conditions that will prove requisite to acceptance of the principles of behavior as articulated by even the ablest scientist; however, and this is consonant with Skinner's view, their acceptance should be enhanced to the degree they are viewed as complementary to ultimate survival.

Skinner notes near the end of "Consequences" that the efforts of those who have made experimental analyses of behavior have been rejected. The reason, he states, is that this approach has "no place for a person as an initiating agent." To hold that individuals' behavior is attributable solely to the contingencies of reinforcement of those individuals' lives as the contingencies have shaped and changed their beings to the present is, from the individuals' perspective, to deny responsibility and control over their lives. On the contrary, societies have generally fostered perspectives that do attribute control to the individual, or, rather, societies generally have been selected for espousing such perspectives, probably because they have strengthened their bases for holding individuals responsible for their behaviors in the framework of what is presumed to be "good and bad" for the well being and the survival of those societies.

On the other hand, the possibility should not be ruled out that the trend of human evolution has included selection for those individuals whose views were readily shaped by environmental contingencies to the conclusions (i) that as individuals they live in a cause and effect determined world and (ii) that their behavior can be self-controlled and used causally to achieve reinforcing effects (i.e. goals).

Clearly, persons persist in perceiving their actions as efforts to transcend their environmental pressures and crises so as to become controllers of these forces and thus avoid being controlled by them. Even Skinner asks whether we can "take explicit steps to make our future more secure?"; and he then asks if to do thus "must we not in some sense transcend selection?" Skinner clearly hopes that success will be obtained through certain steps, which would include "showing or telling people what to do with respect to relevant contingencies – or [how to] construct and maintain new selective contingencies."

Sadly, there is little empirical evidence to warrant optimism for success through the initiation of such steps. Such methods have been relatively ineffective to date in building a world in which peace, safety, security, health, and happiness are ensured. Quite the contrary has prevailed, and the picture grows less bright each year.

In short, though many are ready to join Skinner in such steps, the fact is that we do not know how to incorporate behavioral science into the operations of society so that it will be a powerful contingency of its present and future. Surely the appeal of a cognitive psychology, lamented by Skinner, is in part based on its assumption that some control of behavior is retained by intraorganismic operations, though those operations are not held to be apart from the prescriptions of natural laws.

One hopes that scientists will unite quite involuntarily in support of a perspective of behavior to the degree that it is valid. Only if we are educating scientists improperly should it be otherwise. Even so, the question remains, Why should a society want to incorporate a perspective of behavior that places the control of behavior, not in the individual, but solely in the contingencies of reinforcement as they have brought change in the individual to the present point in time? To be zealous about doing so would seem tantamount to justifying efforts to preserve life, as we know it, only to the end that the contingencies of the environment will continue to have something on which to operate and to shape – a somewhat less than compelling raison d'être.

Paradoxically, a valid perspective of behavior, whether at hand or to be obtained in the future, might inherently be unpalatable to society and be viewed as antithetical to its welfare and survival. It might prove to be the case that for a valid

ery of behavior that seems refractory to contingency management or is amenable only to highly specific contingencies is hardly a defeat for the operant approach. The constraints on learning and perceptual or motor processes simply define the context and limit the degrees of freedom in which operant learning may occur.

In conclusion, the operant analysis of behavior provides rapid feedback and encourages one to "go with what works." This basically atheoretical approach is itself sensitive to contingencies and seems *quite* adaptive.

Fitness, reinforcement, underlying mechanisms

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Skinner is right: Natural selection and operant conditioning are two versions of the same phenomenon. This fact explains not only their form, content, and relations to findings in biology and psychology, but also the complete parallel in their respective intellectual histories. In particular, their relation to these findings both explains and enables us to refute persistent though mistaken charges made against the two theories. It also points to the direction in which both of these theories must be extended. Skinner's account of the identity of selection and reinforcement, however, blocks the explanation, the defense, and the elaboration that the two theories require.

The most venerable charge against the theory of natural selection is that it is unfalsifiable: Its claims about differences in fitness can only be substantiated in the differential reproduction it cites fitness to explain. The way to refute this charge is to specify the mechanisms that characterize differences in fitness independent of their effects in differential reproduction. The practical difficulty of specifying these mechanisms on which fitness supervenes arises because they involve an overwhelmingly large number of disjunctions of physical factors that differ from case to case, so that no general characterization of fitness combining precision, manageability, and truth can actually be constructed. However, *once it is seen that in principle* this independent specification of fitness can be accomplished, the charge of vacuity is easily undercut. More important, by exposing the underlying factors that make for fitness differences from case to case the explanatory and predictive power of applications of the theory of natural selection is increased (see Rosenberg 1983).

By reflecting on the relation between fitness differences and their underlying mechanisms among organisms in environments, we may put to rest the venerable charge's latest versions, like Gould and Lewontin's (1979) complaint that the adaptationist program is but a sterile Panglossianism ("everything is for the best"). It is therefore ironic that, as Dennett (1983) has shown, Gould and Lewontin's unavailing arguments against adaptationism are identical to Skinner's arguments against mentalism. It is even more deeply ironic that extending operant theory and defending it against accusations that it mirrors the alleged Panglossianism of evolutionary theory requires that operant theory be developed in a direction Skinner has categorically abjured: the direction, if not of mentalism, then of "centralism," taking seriously the psychological states that intervene between initial reinforcement of emitted behavior and its subsequent recurrence.

Skinner's critics have long chided the law of effect with vacuity, on the grounds that the only general characterizations of reinforcers available make the law an empty tautology. The criticism has warrant in texts that define a reinforcer as "any stimulus which if presented (or withdrawn) contingent on an operant, increases (decreases) the probability of the occurrence of the operant." (Compare criticisms of evolutionary theory spawned by definitions of fitness or adaptation as whatever increases subsequent reproduction.) Of course, the actual laboratory practice of operant psychologists in uncovering particular contingencies of reinforcement leads them to identify particular reinforcers independently of the change in the frequency or character of particular operants. Thus they have produced quantitative instantiations of the law of effect for small numbers of organisms in specific experimental settings. But to unify and theoretically substantiate these findings under a general theory we need to find a feature common to all reinforcers, aside from their operant consequences. Without one we cannot identify and measure reinforcers independently – and therefore we cannot use the general version of the law to explain and predict anything. Independent identification of reinforcers is needed to link the law to something not already identified as a reinforcer or an operant, and such linkage is required for the provision and improvement of explanatory and predictive content. Despite a great deal of contemporary research, no feature common and peculiar to reinforcers has so far been found. This is because what makes a contingency reinforcing must ultimately be some common effect of reinforcers inside the body of the organism. This common effect, or disjunction of effects, will be the intermediate links, the immediate causes of subsequent emission of operant behavior. (Compare evolutionary theory's need for a common denominator of fitness independent of the reproductive effects fitness is intended to explain.)

Now, looking for the causes of behavior inside the body is not by itself under Skinnerian anathema. As Skinner (1964, p. 84) has written, "The skin is not that important a boundary." But looking for causal mechanisms is excluded. Finding the proximate causes of behavior inside the body is not only demanded by the defense and the development of operant theory, but is an exemplification of the "causal pattern of classical mechanics" which Skinner rejects. It is also in his view the first step toward "mentalism."

The signal accomplishment of the theory of natural selection was not to supersede classical mechanics, but to show how the physical sciences can be expected to subsume biological phenomena. It relies on this relation between selection on heritable variation on the one hand and the forces and factors of physics and chemistry that fitness consists in on the other, to defend itself against charges of vacuity. It dispenses with creativity by appeal to prior noncreative phenomena; it accords to living systems the same essence it accords to physical ones. If, as Skinner rightly claims, operant theory has all the strengths of natural selection, it must have these properties as well. Just as evolutionists have progressively found the required causal intermediaries between selection and evolution, so operant psychologists must find causal intermediaries between reinforcers and operants. Only such intermediaries can satisfy the physical requirement of no action at a (temporal) distance, and provide the linkage to nonbehavioral factors that entrench and substantiate the law of effect. Operant psychology must find a mechanism parallel to the physical factors on which fitness supervenes. This is the course of development that Skinner wants to exclude, however. He rejects the "attempt to assimilate selection by consequences to the causality of classical mechanics." "People," he writes in "Consequences," "behave in given ways because they have been changed by . . . [reinforcing] contingencies. The contingencies can perhaps be inferred from the changes they have worked, but they are no longer in existence." But the direct inference back from changes in behavior to prior contingencies of reinforcement is too easy to have much content; in this respect it is like facile inferences from current adaptation to prior selection, inferences that the opponents of adaptationism have seized upon. In both cases, causal intermediaries are required.

What will these intermediaries look like? Some of Skinner's arguments against mentalism are good ones, and suggest convincingly that we should not look to "folk psychology" for these

cannot be different from that of the biologist. What is different is that causal sequences in physical systems are typically linear, whereas causal sequences in living systems are invariably circular. The circularity, however, is not static. Living systems are a set of processes moving in time and tied together by a nexus of causal sequences. Each process is a causal influence on some other process, and in a system with a limited number of such processes, each eventually feeds back on itself. Such circular causal sequences might more accurately be labelled as spiral.

The failure to note *this* difference leads to a failure to see the reasons and consequences of such spiral causal sequences. The error then ripples through the whole conceptual structure that Skinner attempts to build in "Consequences."

There are at least two reasons for this pervasive circularity of causal sequence. One is that phenotypes are not mere passive vehicles for genes and victims of natural selection. Phenotypes are operators (Waddington 1969) in that they alter their environments in many different ways and hence change the selection forces that act upon them. This is how the differential propagation of genes occurs. It is pointless to ask which is prior, the nature of the environment or the activity of the organism. They are inextricably bound together in what Lewontin (1982) calls the "*interpenetration of organism and environment*" (p. 159). It is simply incorrect to offer a dichotomy of either "a species adapts to an environment" or "the environment selects the adaptive traits." The error is compounded by asserting that the latter is correct. Neither is correct – nor are any of the other alternative interpretations that Skinner offers for a range of biological and psychological issues. What happens is that a species, in adapt*ing* (it is never adapt*ed*) to an environment changes that environment, which then requires further processes of becoming adapted, which in turn imposes more environmental change, and so on. Exactly the same applies to the individual learner. Thus learners *are* doers, and sometimes what they do occurs largely in their heads. In doing they change themselves and their worlds. Piaget's dialectic of assimilation and accommodation captures this endless interplay more nearly than any other formulation. Skinner's static view doesn't begin to approach the complexity of living things.

The other source of circular (spiral) causal sequence is the hierarchical organization of living systems. Genes partly determine phenotypic features, and phenotypic fitness determines the differential propagation of genes. Several different hierarchies are available in the literature. A serious problem for theoretical biology is that the obvious phenomenological, structural hierarchy (macromolecules, organelles, cells, tissues, organs, organisms, ecosystems), which everyone recognizes, may not match the hierarchy of dynamic processes that embodies the *functioning of living systems*. A similar point has been made by Dawkins (1978), Hull (1980), and ourselves (Plotkin & Odling-Smee 1979), and it is not trivial because the obvious hierarchy may get in the way of our seeing the functionally important one. An example of a nonobvious hierarchy is the one we have put forward. Genetic processes, variable development, individual learning, and socioculture at no level map out in any simple way onto an organism. Each level partitions individuals or aggregates them. This conceptual difficulty aside, if one attempts to analyse such hierarchies in any depth, circular causal sequences immediately become apparent in the form of "upward" and "downward" causation (Campbell 1974a). An example of two-way causation in such a hierarchy occurs when genes are important and inevitable determiners of how and what is learned, but what is learned is often an important determiner of what genes are fed back (downward) into the gene pool.

The interconnectedness of living systems may be complex, but interconnected they are. It is the characteristic and essence of living things. The levels never become decoupled. Skinner is in error when he writes that "operant conditioning could not only supplement the natural selection of behavior, it could replace it." The logical requirement of hierarchical organization, however, is that learning as a more subordinate process in the hierarchy can never be decoupled from the less subordinate processes in the hierarchy. Learning of any and every sort must be primed by more fundamental processes, be they genetic or developmental. Skinner's is the erroneous thinking that underpins general process learning theory with its now invalidated notion of stimulus, response, and reinforcer equivalence. This same erroneous idea of a single and unencumbered learning process is what underlies Skinner's continuing and astonishing assertion that language and its social correlates are "simply the extension of operant control to the vocal musculature." Oh Chomsky, where are you now! Writing, we hope, a commentary for this BBS issue. [See Chomsky: "Rules and Representations" *BBS* 3(1) 1980.]

There is an unsettling similarity between Skinner's levels 1, 2, and 3 and our levels 1, 3, and 4. It is unsettling because Skinner does not arrive at his levels by analysis, and as a result they appear to have very little in them and the structure as a whole has no conceptual force. By his own assertions, his levels contain no stored information (actual or metaphorical) and no organization. How, then, are they to be explained and what do they explain? According to Skinner "at all three levels the changes can be explained by the 'development' of contingencies of selection." Can they?

Our challenge to Skinner is to abandon easy assertion and take up some kind of analysis of his levels in terms of their interrelationships and their processes. If he can do so without having to have recourse to concepts of information, storage, and organization then all the more interesting. The point, though, is to show us, not just tell us.

Contingency-governed science

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Skinner has provided an insightful and useful analysis of contingency-governed phenomena in the biological and behavioral sciences. "Consequences" is more than a clever attempt to build bridges between operant psychology and indifferent or often hostile disciplines in the biological sciences such as ethology. It reveals a common, unifying theme that runs through most of the biological and behavioral sciences, the notion that selection by consequences, in some form, is a feature of all living things. The organism is viewed as a theory of its environment. Skinner does an admirable job of developing his argument. I would like to comment on a complementary issue, the role played by contingencies in shaping an operant science of behavior. The focus on contingencies encourages self-correction by investigators, a characteristic that may be the most powerful recommendation for the operant approach. This property of contemporary operant psychology may be a surprise to those who confuse the behavior-analytic approach with the radical environmentalism of a half-century ago.

If an operant psychologist finds that contingencies under his control fail to influence the behavior of an organism, he may conclude either that the behavior is refractory to contingencies or that the wrong contingencies were tried. In either case, the investigator is forced to reexamine and adjust his method and approach. Recent challenges to the operant position usually fail to consider this property of self-correction, and that operant psychology is not a form of environmentalism. Three such challenges are the discovery of "biological constraints on learning," "feature detectors" in sensory systems, and "pattern-generating circuits" in motor systems. All would structure and therefore restrict the ways organisms learn, perceive, or move. I presume that Skinner would say that all three evolved in response to the contingencies of natural selection. The discov-

A one-sided view of evolution

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Skinner draws an analogy between evolution by natural selection and both learning and social change. This raises two questions. How adequate is his picture of evolution? How good are the analogies?

On the first point, I agree that life is best defined by the possession of those properties – multiplication, heredity, variation – that make evolution by natural selection possible, and indeed inevitable. The other properties of living things, and in particular their apparent adaptedness, follow necessarily from these three. I agree that there is no need for a "vital principle called life." Yet I think Skinner's view of evolution is one-sided. This comes out when he rejects "a species adapts to an environment," in favour of "the environment selects the adaptive traits."

The environment can only select traits that arise in the first place, and hence the course of evolution depends on the repertoire of variation. For example, palms grow intermingled with broad-leaved trees in the same forest, subject to the same environment, but their forms are characteristically different. This is because their mode of growth is different: Palms have never evolved the device of "secondary thickening," whereby the trunk thickens by the addition of annual rings of growth. So far as we know, there is little limitation on the kinds of changes that can occur by mutation in the sequence of DNA in the genome. But changes in DNA have their effects (which are naturally selected) by influencing a complex developmental process. Consequently, only certain variations are possible to a given kind of organism. For example, no vertebrate has ever evolved six legs, handy as an extra pair would sometimes be.

For these reasons, I prefer to think of species as adapting to their environments. I am also puzzled by Skinner's objection to the "storage" of genetic information. As it happens, I dislike the claim that "genes . . . 'contain the information' needed by the fertilized egg in order to grow into a mature organism," but my reasons are different from Skinner's. I object because the phrase suggests that we understand the process of development and its genetic control, when in fact we do not. However, the statement "genes contain the information needed to make all the proteins of the mature organism" has a precise and quantifiable meaning, and is correct (except, perhaps, for antibody proteins). Of course, insofar as there is information in the genome, it is there only because of past natural selection, but it is still there.

Thus I think Skinner's picture of evolution is correct but one-sided, because it ignores the structure and development of the organism, and the resulting constraints on the repertoire of variation, and it ignores the mechanisms whereby genetic information is stored and transmitted. I suspect that this one-sidedness reflects an analogous one-sidedness in his concept of learning and behaviour. Thus in his theory of learning, there are no constraints on the kinds of actions an animal may try out, so that the end result depends only on the pattern of reinforcement, just as in his picture of evolution there are no constraints on variation. Similarly, in Skinner's theory of behaviour, there seems to be nothing in the animal's head, just as there is no storage of information in his picture of the genome.

Thus his analogy between natural selection and operant conditioning is a good one, but it is an analogy between a one-sided theory of evolution and a one-sided theory of learning. I can understand the wish of psychologists to study behaviour while ignoring neurophysiology. Analogously, Weismann's (1889) concept of the separation of germ line and soma made it possible to study genetics and evolution without understanding development, and considerable progress has resulted. But it does not, or at least should not, lead biologists to think that development does not happen, or that the structure of organisms does not place constraints on future evolution. In the same way, it may sometimes be fruitful for psychologists to treat the brain as a black box, but that is no excuse for claiming that the box is empty.

Skinner's second analogy is between biological evolution and culture: "It is the effect on the group, not the reinforcing consequences for individual members, which is responsible for the evolution of . . . culture." For this to be true, we must suppose the following. There are a number of different human groups, each with a different culture. The culture of a given group changes from time to time, in ways unrelated to the overall trend of cultural evolution (just as mutation causes changes unrelated to the trend of evolution). The cultural changes that do occur alter the chances of extinction, survival, and splitting of the group. The overall trend of cultural change is determined by this differential survival. Groups will therefore tend to have cultures that ensure their survival.

I have spelt out this argument more explicitly than Skinner has done, because I want to be sure I understand him. I'm not sure he would go as far as I have gone, but unless the assumptions of the last paragraph are correct, then the analogy between evolution and cultural change is a misleading one. In prehistoric times, when there may have existed a number of culturally isolated groups, the process outlined may have played some role in directing cultural change. As a causal explanation of, for example, the change from the feudal England of the Middle Ages to the England of today, it will not do, if only because the requisite group structure has not existed. For example, during my lifetime there have been dramatic changes in attitudes towards birth control and abortion. For these to have been brought about by a mechanism of the kind Skinner proposes, society would have to consist of a series of groups, some practising birth control and others not, the former being more successful in dividing to form new groups: This is manifestly not the case.

To sum up, Skinner's analogy between natural selection and operant conditioning is a close one, but it is made possible only by the one-sided nature of his pictures of evolution and learning. His analogy between cultural and biological evolution seems to me to be of little value.

Linear and circular causal sequences

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Skinner seems wrong on almost every point that he makes in "Consequences," with one important exception. That is that selection *is* the basis by which living systems gain knowledge of themselves and their world. Selection operates at the genetic, developmental, individual learning, and cultural levels (see Plotkin & Odling-Smee 1981). How then could he be wrong about everything else? It is, we think, because he makes a fundamental error in asserting that selection by consequences is a "causal mode" (whatever that is) which is somehow different from other causes – indeed he contrasts it with "the causal pattern of classical mechanics." It is hard to know what Skinner means by this because he is not explicit, but the implication seems to be that causation in biology and the social sciences is somehow different from that in the physical sciences. He is, of course, in very good company. Mayr's assertions (1961; 1982) that ultimate causes (changes in the genetic program brought about by natural selection) and proximate causes (the expression of the genetic program in phenotypic form) are different, constitute a similar claim. But it seems to us that causation cannot be arbitrarily divided in this way. The causation of the physicist

may well have selected a capacity in animals for developing anticipations that guide appropriate behavior and enhance the likelihood of survival. The maintenance of behavior through anticipatory and associative mechanisms may well be parallel to the genetic template, and to the maintenance of social custom through rules, codes, and other directives.

Cause and effect in evolution

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Professor Skinner argues that the analogy between evolutionary processes in biology and learning processes in behavior is a good one, and, along the way, he bares a number of epistemological assumptions common to biology and to psychology. Central to his discussion is a particular sequence of causes and effects, the unfolding of which produces "selection by consequences" – an *overall* effect with a somewhat peculiar flavor. Perhaps a more extended analysis of this idea can be even more revealing.

"Selection by consequences" – generally called natural selection – is the broad-scale effect generated by a particular underlying sequence of events. What exactly is this sequence? Usually, natural selection is thought of in terms of populations, but for simplicity let me begin with the existence of a single animal, A. Next, we introduce a test – does animal A match some necessary requirement of its environment? If the match between animal and environment is appropriate, then the animal survives. If the match is not appropriate, the animal dies. Thus, the basic sequence of events is: existence of A, test of matching, existence (or nonexistence) of A.

Natural selection is most often envisioned as a continual process. This means that natural selection consists of an extended iteration of the basic sequence of events, namely: . . ., existence of A, test of matching, existence (or nonexistence) of A, test of matching, existence (or nonexistence) of A, test of matching, ... Furthermore, in the biological world there is one additional step added to each iteration of the basic sequence: Animal A may change. Therefore, the full sequence is really: . . . , existence of A, change to A_1, test of matching, existence (or nonexistence) of A_1, change to A_2, test of matching, existence (or nonexistence) of A_2, change to A_3, test of matching, ...

Where is the cause and effect in this sequence? At any one time, the direct cause of the existence (or nonexistence) of A is a preceding test of matching. Conversely, the direct effect of a test of matching is the existence (or nonexistence) of A. This appears to be entirely consistent with the classical notion of cause and effect – in other words, a sequence of events can be defined such that the preceding event can be considered to be the direct cause of the next event, which, in its turn, can be considered to be the direct effect of the preceding event.

In addition, the statement "change to A_2" contains another implicit cause and effect relation. Although I have not specified it in this abstraction, the common presumption is that changes in A are effects directly brought about by mechanisms (causes), such as mutations, that are entirely consistent with the well-understood laws of the physical world.

On the other hand, as Skinner points out, the idea of natural selection includes something a bit peculiar, something that appears different from the traditional notions of cause and effect as they are usually applied in the physical world. If this new something is not the cause and effect relations themselves, then what might it be? Perhaps it is the highly ordered nature of the *overall* effect that is produced. Specifically, the new idea in natural selection appears to be that although they lack rationality, forethought, or purposive organization, the standard cause and effect relations operating in the natural world nonetheless do not lead to homogeneous or random phenomena. Instead, natural selection results in certain particular, complex, and well-organized phenomena. Here, in fact, is the apparent paradox: How can undirected causes produce apparently directed effects?

There are a variety of ways in which direction is built into the process of natural selection in the biological realm. Let me just mention two of the most fundamental directional forces. First, there are the nature and the specificity of the operative matching test. The particular matching criterion that determines survival can strongly shape the form of the surviving population. Even homogeneously or normally distributed populations can be drastically restructured by certain matching constraints (Katz & Grenander 1982), especially when the sequence of existence, testing, existence, testing, . . . is very long. For example, when bacteria are forced to match very peculiar environments, natural selection can readily produce homogeneous bacterial populations with very peculiar biochemistries.

Second, there is the intrinsic nature of the precursors themselves, which can strongly direct the step "change to A_1." Intrinsic constraints mean that A can change into only a certain select set of A_1s. Although certain matching tests can produce unusual bacterial populations, these populations are only amplifications of the limited potentials of the bacterial genome. For example, it appears that bacteria do not have the intrinsic potential to develop mitochondria; therefore, it is unlikely that natural selection can readily produce bacteria with mitochondria. Moreover, although almost any part of the genome of an organism can, in theory, change, many of the potential mutations cannot actually be incorporated into a viable organism. And of those mutations still compatible with a viable organism, some will be otherwise detrimental and others will be effectively invisible.

In most biological situations, natural selection operates on a complex precursor. Complex precursors have, by definition, a great many possible features to change. However, the highly interactive nature of most complex biological entities further constrains the actual changes that can be successfully instituted (Katz 1983). Although not preplanned, all of these intrinsic constraints end up channeling and thereby giving direction to the overall effects that are produced in long causal sequences of natural selection.

Sometimes the intrinsic constraints are readily apparent in the systems that are undergoing natural selection. Frequently, however, the intricacy and the complexity of biological systems make it difficult to distinguish immediately the inherent directional effects. This is especially a problem in those cases, such as most multicellular organisms, in which the systems are composed of a great many different interacting elements, all balanced in a dynamic equilibrium. Here it is necessary to perturb the system in a controlled manner to reveal many of the influences that direct the causal sequences of natural selection.

These controlled perturbations are actually "evolutionary experiments," because the causal sequences of natural selection – composed of the repeated iteration "existence of A, change to A_1, test of matching" – are synonymous with "evolution." As Skinner has emphasized, essentially the same evolutionary process found in the realm of natural selection among organisms can also be seen in the realm of behavior and in the realm of culture. Such evolution is not peculiar in its cause and effect relations. Rather, it is peculiar in its apparent directedness. Evolution is truly directed, in the sense that it flows along in only certain channels. Professor Skinner reminds us that although the complex order that is thereby created is wonderful, it does not countermand any natural laws.

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that characterize cultural evolutionary processes. The other, selection of behavioral innovations in individual repertories is far more common. In fact, group selection is merely the limiting case of individual selection in which the consequences are catastrophic for all group members. Sociocultural systems usually change well before catastrophic consequences lead to group extinction. One has merely to cast an eye at the rapid pace of changes in technology and domestic life to get the point. Automobiles and electric lights were not selected for as a consequence of their contribution to group survival (cf. "contingencies . . . promoting behavior which contributes to the survival of the group") but because they constituted reinforcements for specific individuals whose behavior was thereby shaped.

When we say that behavior has been selected for as a result of its favorable consequences for a group, we can only mean that it has had favorable consequences for some or all members of the group sufficient to outweigh its adverse effects on some or all of the members. The cumulative shaping of individual behavior is precisely what cultural evolution is all about. Of course, these behaviors are interrelated and in conjunction with various environmental and social feedback processes possess systemic properties that are the logico-empirical basis for the concepts of society, culture, and sociocultural systems. For an anthropological behaviorist, events on the sociocultural level are necessarily abstractions (concrete and real) derived from the observation of behavioral changes in individuals, and the evolution of sociocultural systems is necessarily the evolution of such behavior.

Thus human behavioral repertories consist overwhelmingly of operantly conditioned responses that are at the same time culturally conditioned responses, that is, responses shaped in conformity with culturally determined reinforcement schedules and contingencies. Therefore Skinner's claim that "the contingencies of selection at the three levels are quite different" is incorrect in the human case as regards levels ii and iii. In the human case the contingencies of selection are not random but occur in conformity with programs encoded primarily in the brains (or other neural pathways as distinguished from the genes) of enculturated individuals (and not, as Skinner proposes, merely in "documents, artifacts, and other products of . . . behavior").

Behaviorist principles can tell us how these individuals shape each other's behavior, but they cannot tell us what behavior they will shape. Skinner's criterion for separating levels ii and iii in the human case obscures this problem and deters fruitful collaboration between materialist, behaviorist, nomothetic anthropologists and like-minded psychologists.

Practitioners of the science of culture need to know more from psychologists than the general laws of operant behavior. In order to predict or retrodict favored or unfavored innovations in cultural repertories and hence to understand the divergent and convergent (not merely unilinear) trajectories of sociocultural evolution, we need to be able to measure cost–benefit consequences as "currencies" relevant to the biologically determined discriminative stimuli and biologically determined reinforcers that underlie operant conditioning in the human case (i.e. innate biopsychological drives, needs, instincts, etc.). Without such knowledge we cannot specify the consequences of behavioral innovations and hence cannot operationalize the principle of selection by consequences.

On the stabilization of behavioral selection

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The processes of change described by Skinner in "Consequences" are clearly 'retrospective'; neither successful nor unsuccessful changes are selected in an anticipatory fashion. Genetic alterations are not decided in advance by environmental pressures; the strengthening or weakening of behavior is not carried out in anticipation of its outcomes; social structures are not established with the intent of effecting social change. Where there is an apparent anticipation of successful change, this is owing to biases (of the form of responding, for example) or to "rules" that have themselves been selected in retrospective fashion. In fact, much of Skinner's argument was anticipated in a provocative article by Donald T. Campbell (1960), concerning variation and selective retention in cognitive and creative behavior.

While we recognize the retrospective action of the consequences of genetic or behavioral or social variation, we must also note that these changes each incorporate mechanisms for stability that oppose further change. In the case of evolution, this is perhaps a trivial point; a morphological change is permanent until modified by a further, presumably random, successful mutation. The change is "stored" genetically. Social change is "codified" and transmitted as law or tradition (Tevye's sons in *Fiddler on the Roof*), and the retrospective origins of the change are readily forgotten by those who are controlled by social institutions. But how is behavioral change "fixed" and used by individual organisms? Skinner is, as ever, silent on this question. The functional definition of reinforcement as a retrospective process cannot at the same time explain the action of reinforcement in fixing behavior.

Skinner suggests that the reinforceability of behavior is itself a consequence of evolution, because adaptiveness of behavior enhances survival value. It is equally reasonable to suppose that animals evolved mechanisms for rendering the selected behavior resistant to change. The physiological forms of such mechanisms still need to be identified, but students of animal learning seem generally to agree that "associations" are formed either between the behavior and its consequences (instrumental conditioning) or between an initial signal and a primary or unconditioned stimulus that elicits a response (classical conditioning). The nature of association as a psychological concept has been the object of intense study in recent years. Much current evidence suggests anticipatory mechanisms both in instrumental learning and in classical conditioning. For example Breland and Breland (1961) showed in their study of the "misbehavior" of organisms that the instrumental response would "drift" toward the consummatory response required by the reinforcer that follows the response. Likewise, in autoshaping (a classical conditioning procedure) the responses elicited by the conditioned stimulus take a form that is appropriate for consumption of the reinforcer (Jenkins & Moore 1973). In fact, classical conditioning is generally viewed as the development of an anticipatory process. Skinner has never claimed that the change of behavior that is part of classical conditioning results from selection by reinforcement following the conditioned response.

Many other findings suggest the importance of anticipatory mechanisms in learning. The "blocking" of conditioning when a second CS is redundantly added to an established CS (Kamin 1969) is usually explained by the fact that the UCS can be anticipated from the latter; only when the outcome of the trial is "surprising" does the second CS gain control of the conditioned response. Likewise, the "value" of a reinforcer can be enhanced or reduced "off baseline," and this will subsequently be reflected in the performance of the acquired response that precedes the reinforcer (see Adams & Dickinson 1981).

Similar findings support anticipatory processes in the production of established behavior; in particular, animals seem to anticipate temporal durations and delays (see Honig 1981, for a review). In "short-term" memory procedures, performance based upon an initial stimulus in a trial is markedly enhanced when different outcomes can be expected following different initial stimuli (Peterson, Wheeler & Trapold 1980). Evolution

engine, and universal suffrage being obvious examples. Here, too, the desired analogy between social and biological evolution breaks down because in the case of social evolution there is a dialectical interaction between innovation and society, by which innovation is a function of society, and in turn changes society.

For this reason to talk of "selection by consequences" fails to grasp that "consequences" not only "select" but can provide *new* opportunities and new problems as the basis for further change, and are therefore themselves an integral part of the process of change, not simply the *conclusion* to change, as Skinner presents them.

With these considerations in mind the apparent emptiness of what Skinner has to say about change and stability becomes especially clear:

> Why do people continue to do things in the same way for many years, and why do groups of people continue to observe old practices for centuries? The answers are presumably the same: Either new variations (new forms of behavior or new practices) have not appeared or those which have appeared have not been selected by the prevailing contingencies (of reinforcement or of the survival of the group). . . . change is explained as due to new variations selected by prevailing contingencies or to new contingencies.

In short, either people go on doing what they have always done, or they do not, and innovations may occur in existing circumstances or in new ones!

Another fundamental defect in Skinner's account of social evolution derives from his failure to come to terms with the notion of structure or organization. On the one hand he seems to resort to individualism, as when he says that the evolution of social environments or culture "presumably begins at the level of the individual. A better way of making a tool, growing food, or teaching a child is reinforced by its consequences – the tool, food, or a useful helper, respectively." He also denies to structure or organization any causal efficacy, and says that "organization and the effects attributed to it can be traced to the respective contingencies of selection." It should also be noted that although he frequently refers to contingencies of selection, and to circumstances that "select the cultural practices which yield a solution" to a problem, and to a situation that "shapes and maintains adjusted behavior," and says that after introducing "new cultural practices . . . we must wait for selection to occur," in all these cases the circumstances and situations which are presented so impersonally actually consist, causally speaking, of the behavior and dispositions of *people*.

But, on the other hand, he wishes to treat cultures or societies as real entities that are comparable to biological organisms, some of which are more effective than others and which will therefore be subject to natural selection. Thus he says "it is the effect on the group, not the reinforcing consequences for individual members, which is responsible for the evolution of . . . culture." One can appreciate that even Skinner might shrink from "explaining" slavery by claiming that owning slaves is reinforcing for the masters, and that submitting to slavery is reinforcing for the slaves. But having dispensed with the notion of structure as a distinct factor in social evolution he seems to be left with nothing better than the old structural – functionalist, holistic notion of societies as real entities with goals and needs of their own distinct from those of their members. So cultures can, for example, "induce individuals to suffer or die as heroes or martyrs."

The solution to this dilemma is to recognise that although of course structure or organization cannot by itself *do* anything, and that only real, individual people have any *causal* powers in space and time, the individual members of a society are not causally *autonomous*. That is, what they do and why they do it are also expressions of the institutions, categories, rules, beliefs, and values of the particular society into which the individuals composing it have been socialized, which they did not create as individuals, and which will outlast them. Now, while these institutions and cultural forms cannot do anything, they certainly possess structural properties, as objective as those of the natural world, and it can easily be shown that some institutions will not fit with others, or can be elaborated in certain directions and not in others. In the same way, belief and value systems have an internal logic of their own, and certain innovations will not work because they are inconsistent with the basic principles of these systems. Social evolution is, among other things, a process of exploration of the objective properties of social and cultural structures. By ignoring the objective properties of structure, Skinner deprives himself of any means of bridging the gap between the characteristics of individual behavior and those of society.

The statement by Skinner in his abstract that "social behavior is within easy range of natural selection, because other members are one of the most stable features of the environment of a species" is therefore profoundly incorrect, because it totally ignores the dialectical interaction between individual behavior and sociocultural structure.

Skinner maintains that "selection by consequences" is superior to other theories because it refutes "the supposed origins of a culture as a social contract or of social practices as commandments" and also disposes of theories of "group minds" and "zeitgeists." Must one point out that relatively few of us in the social sciences believe that societies were created by wild men emerging from the forests and shaking hands, or by the fiat of culture heroes, and that theories of group minds and zeitgeists have long gone the way of the Absolute as serious subjects for debate in social evolution? Not only is it unnecessary to use Skinner's theory to refute these ideas, but it is far from obvious that "selection by consequences" could refute them anyway.

The search for parsimonious and general theories is all very well, but such theories must also be adequate to the facts, and the facts of biological evolution, individual psychology, and social evolution are so vast and diverse that there seems no good reason to believe that *any* general law could encompass them all. As the law of "selection by consequences" illustrates, the result of such an endeavor is likely to be a combination of the trivial and the profoundly misleading.

Group and individual effects in selection

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Anthropologists would benefit from arguments on behalf of selection by consequences as a metaprinciple for explaining cultural as well as biological evolution and the acquisition of individual response repertories. Contemporary anthropology (with the exception of archaeology) is not only "heavily structural" as Skinner states, but heavily ideographic, emic, voluntarist, mentalist, and even mystical or obscurantist (Harris 1979). Since I wish to be none of these, the critical remarks that follow should not be viewed apart from my fundamental agreement with Skinner's positivism and materialism and my own intellectual grounding in reinforcement principles as taught by William Schoenfeld and Fred Keller (Keller & Schoenfeld 1950) many years ago.

"Consequences" is flawed by the slipshod manner in which Skinner characterizes the contingencies responsible for cultural selection (iii) and the nature of behavioral selection (ii) as it applies to the human case. The author states: "It is the effect on the group, not the reinforcing consequences for individual members, which is responsible for the evolution of culture." This is both an epistemological lapse (unoperationalized entity) and counterfactual. Effects on the group are aggregate effects on the individuals in the group (Harris 1964). This is not to deny the occurrence of group selection in cultural evolution, but to identify it exclusively with extinctions of regional or local repertories caused by war, famine, and other catastrophes. Group selection is only one of two forms of selection by consequences

opportunity space? There are also questions about the mode of retention of selected variations, and concerning conflicts between individual selection and group cooperation. Another of these important issues is whether all selection takes place in regard to direct contact with contingencies or through vicarious selection systems such as thought trials. These are all important challenges to the research program that the interested reader should be aware of.

The emancipation of thought and culture from their original material substrates

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We should be grateful to Skinner for his attempts to purge psychology of unnecessary metaphysical encumbrances. Getting rid of a vital principle of life and its analogues is conducive to clear thinking and to the effectual solution of legitimate scientific problems. Yet, especially in the light of developments in evolutionary biology over the past few years, I wonder if some of his efforts have perhaps led to oversimplification.

For one thing, those of us who have been working on adaptation over the last two decades have learned not to ask what is good for the species or anything else. It was long forgotten that what organisms do happens because of differential reproduction among other organisms in ancestral populations. Right thinking means asking, not what is good, but what has happened. Otherwise we are apt to misconstrue the underlying mechanisms, with unfortunate consequences. This should apply to natural selection, learning, and cultural transmutation.

Skinner's analysis of cultural evolution is a case in point. He treats culture as if it were identical with verbal behavior, implicitly embodying it in the organisms who behave verbally. This implies that the culture *is* those organisms, and that anything exterior to them constitutes part of the culture's social environment. The organisms themselves, and groups composed of such organisms, would be the replicanda which, by analogy with ordinary biological evolution, are selected and evolve. This gives us a model of cultural evolution that links the survival of the culture to the survival of its biological substratum. Evidently Skinner wants to treat culture as a class of verbal behaviors, inseparable from a class of verbally behaving organisms. Unfortunately "culture" is a mass noun, and it is not clear what the individuals are. As I see it, culture is a class of cultural individuals, such as words, sentences, and languages. Its connection to organisms is accidental, not necessary. (See Ghiselin 1980; 1981; 1982.)

Just as the gene (in two senses) is the replicandum and also the lineage of replicated genes, and just as the species is the nexus of successive generations of parents and offspring, so the culture is the totality of replicated individuals and all of their descendants, which itself forms a larger individual, lineage, and whole. In other words, culture is made up of everything that is produced through behavioral replication and its indirect consequences. It evolves through selection of those products, not necessarily of their producers. These products include artifacts. For example, someone writes a book. It is copied, revised, duplicated, cited, perhaps translated, imitated, and even plagiarized. It might spawn a lineage of similar works – a genre even. At any rate, the book is not dependent upon any particular piece of matter in which it happens to be embodied. Equating culture with verbal behavior is like failing to distinguish between literature and publishing. The medium definitely is not the message. An element of a book (a copy) is no more a receptacle for culture than its author is. The cultural whole is incarnate in both. By the same token it is erroneous to claim that the artifacts are the "environment" of the culture. Like the organisms that make up a species, they are integral parts of it. The author may die without biological issue: Yet his literary child may endure for ages. In this sense, at least, culture may be said to have a life of its own.

On the other hand cultural entities, like organisms, do form parts of environments in both biological and cultural evolution. A myth can function as a selective influence just as much as a predator can. We fear both, and act upon it. When we consider how the nervous system operates, it is not clear how we should extrapolate to it from other entities subject to selection. That some kind of parallelism exists, as Skinner maintains, seems eminently reasonable. We might nonetheless ask just how far the genes possess hegemony over the intellect. The automaton theory of behavior would have it that the soma is a mere puppet to the germ – or to some antecedent condition of the soma. Yet if culture can be autonomous – and a selective agent in its own right – why not thoughts? Is the mind nothing more than the slave of the gonads? We cannot evade this issue merely by complaining about the metaphorical language. However we choose to express it, we have a substantive issue about how behavior relates to that which behaves. Skinner may have gone too far beyond freedom and dignity.

Fitting culture into a Skinner box

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As an anthropologist, I would like to address those aspects of "Consequences" that seek to apply the notion of "selection by consequences" to social and cultural evolution.

The first and perhaps most fundamental objection is that, whatever "consequences" may mean, *something* has first to come into existence before it can be "selected" by them. The sources of novelty are just as important as the success or failure of novelty, but Skinner has nothing to say about the sources of social innovation except that they are variations that are reinforcing to the individuals who introduce them. This attitude to innovation is not, of course, surprising in someone who believes that creativity is nothing more than random variation of existing procedures, and who is trying to show that a simple model – "variation proposes; environment disposes" – will apply equally to biological evolution, individual learning, and social evolution. The model, however, is totally inadequate in the face of social reality. For example, the supersession of stone tools by metal tools is a clear instance of "selection by consequences," but the reasons for preferring metal to stone tools are perfectly obvious: Metal tools do not break, can be easily resharpened, and are far more versatile in shape than stone. The real problem is *not* to explain why metal was preferred to stone but to understand how and why metal technology originated in the first place, and it is therefore sheer mystification to treat innovation as "random variation."

Unlike biological mutations, which do not occur as responses to the environment, and are not under the control of the organisms in which they occur, social and cultural innovations are conscious responses to certain aspects of the organisms' social environment. In the case of social evolution, therefore, we cannot operate on the basis of that neat separation between variation and selection that is favored in neo-Darwinian biological theory. Although social innovations are initially produced by individuals, individuals do not innovate in a vacuum, or in some entirely private and idiosyncratic world of their own, but as members of a particular society at a particular period in history.

Again, although organic mutations do not change the physical environment of the organism, social innovations very definitely do change their social "environment" – printing, the steam

is self-described as a "triumph of rationalism over empiricism" (Katz & Bever 1976, p. 10).

The arena in which these values most openly conflict is, as Skinner notes, the treatment of complex human behavior – especially memory and language. (It should be noted that even Wallace, the cooriginator of the theory of evolution through natural selection, demurred in its application to the human species.) That philosophical differences are at stake is illustrated by comparing the treatment of reinforcement by a modern linguist (Chomsky 1959) with that of natural selection by a 19th-century linguist (Müller 1872). Present-day generative grammarians posit universal linguistic rules of such an abstract character that their origins are claimed to be beyond the impoverished input afforded by the contemporary environment. Reinforcement is therefore said to be incapable, in principle, of engendering language. In its stead, an appeal is made to a genetically based universal grammar resulting from natural selection (Chomsky 1980a, pp. 263, 321; 1980b, pp. 3, 9). Nineteenth-century linguists also characterized the defining features of language as universal and abstract. Moreover, the same philosophers – Plato and Kant – are favorably cited by both generations of linguists (Chomsky 1966; Weimer 1973). Although their characterizations of language were highly similar, 19th-century linguists reached a very different conclusion: They concluded that language was, in principle, beyond the reach of natural selection! What is common to both eras is a resistance to selectionist thinking for as long as the evidence permits. [Cf. Chomsky, "Rules and Representations" *BBS* 3(1) 1980.]

Although most apparent in the treatment of complex human behavior, nonselectionist thinking also continues to leave its mark on the interpretation of simpler learning processes in animals, albeit more subtly. Consider an influential current account of conditioning with the Pavlovian procedure in which the conditioned response (e.g. salivation) is said to be acquired when there is a discrepancy between the asymptotic association value supportable by the unconditioned stimulus, or reinforcer (e.g. meat powder in the mouth), and the initial association value of all contiguous environmental stimuli, notably the conditioned stimulus (e.g. a tone; Rescorla & Wagner 1972). This description of the conditioning process has an implicit teleological flavor: A *future* event, the asymptotic association value, is required as a reference point from which the discrepancy is measured. Even if the learner were endowed by natural selection with "foreknowledge" of the asymptotic association value of all potential unconditioned stimuli – a large order in itself – how such information would be available for all potential learned reinforcers remains a puzzle.

Although I believe that a precise parallelism exists between natural selection and reinforcement as selectionist accounts of organic change, Skinner's article "Consequences" contains two potential impediments to the acceptance of reinforcement as the transcendent principle of ontogeny. First, although culture is a crucial influence on human behavior and reinforcement contributes centrally to an understanding of that influence, the mode of action proposed by Skinner is problematic. The appeal to a new "kind" of selection involving an "effect on the group, not the reinforcing consequences for individual members" seems unnecessary. The proposal is reminiscent of the generally unhelpful concept of group selection (Wynne-Edwards 1963) and might better be replaced by a treatment analogous to kin selection (Hamilton 1964) or reciprocal altruism (Trivers 1971) in sociobiology (E. O. Wilson 1975). Second, although the distinction between respondent and operant conditioning probably served an important function historically, it would no longer seem best to describe them as different "kinds" of selection, or different "processes" of behavioral change. After all, the selecting environment responsible for conditionability included neither Pavlov's conditioning frame nor Thorndike's puzzle box. Respondent and operant conditioning might best be regarded as simply different *procedures* for studying behavioral change, procedures that are potentially understandable in terms of a common reinforcement principle (see Donahoe, Crowley, Millard & Stickney 1982). Skinner was prudent to have focused initially on the implications of reinforcement rather than the microbehavioral and physiological mechanisms that subserved the process. (Darwin's ill-fated theory of pangenesis will be remembered as his attempt to identify the mechanism of natural selection.) Nevertheless, it is also well to recall that the scientific acceptance of natural selection as the primary principle of phylogeny did not occur until over 75 years later with the modern synthesis of evolution and population genetics (Mayr 1982).

The assessment of reinforcement as the fundamental principle of ontogeny will probably follow the steep and thorny path taken earlier by natural selection. To quote Darwin (1888, pp. 148–49) in a letter to Huxley, "It will be a long battle, after we are dead and gone." Let us hope that the matter is resolved before our species is "dead and gone" from the potentially harsh verdict of natural selection.

The wider context of selection by consequences

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Many of us in the social and behavioral sciences welcome and applaud Professor Skinner's continual attempts to clarify the nature of causation in "purposive" systems. However, a reader unfamiliar with current extensions of Darwinian models to nongenetic aspects of adaptation (i.e. learning or social evolution) may get the impression that this work is being done primarily by the field known as "the experimental analysis of behavior." This is not the case.

In psychology the analogy between trial and error learning and the selection of unforesightful variations has long been recognized and developed. Campbell (1956; 1974b) traces the trend back to Baldwin (1900), Thurstone (1924), Tolman (1926), Ashby (1952), and many others. Work in this area continues unabated in traditions other than that designated by Skinner. An especially relevant introduction to such work is available in the exchanges between Michael Ghiselin and his commentators in this journal (*BBS* 4(2) 1981). Important psychological work on selection by consequences by Campbell (1956), Pulliam and Dunford (1980), Ghiselin (1973; 1981), Simon (1966), and many others should also be consulted by those interested in the model.

In sociology and anthropology the situation is much the same. Aspects of these fields are characterized by vital and growing communities of scientists working with extensions of the Darwinian model. Mathematical formalisms attempting to model the nongenetic diffusion of cultural traits have been developed and tested by Boyd and Richerson (1980) and Cavalli-Sforza and Feldman (1973; 1981). Other important work in this area includes that of Waddington (1968) and Plotkin and Odling-Smee (1981). Hence the behavioral sciences at Skinner's levels ii and iii may not be as limited in this regard as he suggests.

Skinner's article may also give the impression that modern selection theory generates a relatively unproblematic research program. This is not the case. Although many of us find it the most promising among current alternatives it is inundated with conceptual and empirical puzzles. Some examples include whether one-trial learning is not a more appropriate analogue to the Darwinian model than is the model of gradual approximation. Another has to do with how to conceptualize units of variation. Are they specific movements? classes of functionally or conceptually similar behaviors? mental representations of the

new person learns the song after hearing the pleasing acoustic consequences of the engram. Society is the medium within which the duplication, and hence survival, of the tune, takes place, but the survival, or otherwise, of the whole society is not, on this view, at issue.

Alternatively is it, as Skinner seems to suggest for level iii, whole societies that are the entities that survive or fail to survive, bag and baggage with all their cultural practices? I object to this suggestion on various grounds: It is factual, implausible, and it probably suffers from analogues of many of the notorious theoretical difficulties of "group selection" (Williams 1966): An awful lot of societies would have to go extinct for even a modest amount of evolutionary change to occur. But all I want to do here is to point out that, logically, the application of the model of selection by consequences to the cultural domain has no necessary connection with group survival or extinction in a metapopulation of groups, as Skinner implies. Similarly, if – which I doubt – group survival or extinction were an important kind of "consequence," it might operate at the level of genetic replicators no less than at the level of cultural ones.

I would not dare to criticize Skinner on his own territory of reinforcement learning, but may I close by briefly offering what I hope is a constructive suggestion? It concerns that ethological chestnut, the "displacement activity." Why does an animal, when "frustrated," "thwarted," or "in conflict," perform an irrelevant act, scratch its head, say, or preen its wing? McFarland (1966) reviews the theories, including his own ingenious theory of attention switching, which seems to me to lead to the following functional hypothesis: Displacement activities may be to Skinner's level ii what mutations are to level i.

If a selective process is to result in improvement it must have variation upon which to work: genetic mutation in the case of ordinary Darwinian selection. The variation offered is random with respect to improvement. The Darwinian theory predicts that, since mutation is a recurrent phenomenon, the mutations we see should mostly be deleterious – the good ones having been already selected into the gene pool long ago – and the prediction is fulfilled. But it is still true that although particular mutations are nearly all deleterious, the phenomenon of mutation itself is vitally necessary for continued evolution. It has therefore frequently been suggested that mutation rates themselves might be adaptive, boosted in evolution in the interests of providing raw material for further evolution. This would have to work by selection favouring "mutator genes," genes whose consequence is to raise the general mutation rate of the animal. Mutator genes exist, but for various reasons the theory is probably wrong (Williams 1966): The optimal mutation rate favoured by selection on mutator genes is probably zero – an optimum fortunately never reached. But the boosted optimal mutation rate theory is wrong only at level i; maybe an analogue of it is valid at Skinner's level ii.

Consider a pigeon in one of Professor Skinner's boxes, under an extinction regime. It has been a statistical law of its world that if you press the red key you get food, and now the law is being violated: No food is forthcoming. What does the pigeon do about it? It preens its feathers, and if an ethologist happens to be looking he will label the movement a displacement activity because it is obviously irrelevant to the task in hand: Any fool can see that you cannot get food by preening. But wait. Any fool might have said that you can't get food by pecking at bits of red plexiglass, and yet the experimenter set up a world in which that was precisely how you did get food. In the world of nature, the big Skinner Box out there, a bird cannot predict what will be good: If it could, it wouldn't need to *learn* what to do, it would just get on and do it. The whole point of level ii selection by consequences is that it can solve problems that level i selection has not solved.

If the animal is frustrated or thwarted, say, because it can see food under the glass, it is clear that whatever solution to the problem it is attempting, say pecking at the glass, is not working. There may in fact be a good solution to the problem – hook the beak under the glass and tweak it off the food – but the pigeon cannot be expected to know this, since neither it nor any of its ancestors has met the problem before. Reinforcement learning is designed to discover the solution to such problems by its special application of the general method of selection by consequences, but it cannot go to work unless there is "mutation" – random production of spontaneous behaviour. No doubt such "mutations" may be produced at any time. But there is obviously something to be said for boosting the "mutation rate" at *particular* times, times when there is a problem at hand and it is not being solved: times, in other words, of thwarting and frustration. So the pigeon boosts its rate of spontaneous behaviour production during times of frustration, the very times when displacement activities are said to occur. If the resulting behaviour should happen to be the correct solution to the problem, the watching ethologist says "clever bird," and the thought of displacement activity does not cross his mind. It is only when the bird does not immediately hit the solution, when it preens itself instead, say, that the ethologist says "Aha, displacement activity." But as far as the bird is concerned, both may be manifestations of the same thing: turning up the spontaneous random "mutation" generator in response to thwarting. Just as most mutations are failures, so too, by definition, are displacement activities. It is only the failures that qualify to be called displacement activities. But that there should sometimes be failure is of the essence of selection by consequences.

Skinner – The Darwin of ontogeny?

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Skinner proposes that the contemporary environment has a selecting effect on individual development (i.e. ontogeny) in a manner that is functionally equivalent to that of the ancestral environment on species development (i.e. phylogeny). The principle of reinforcement is intended to describe the environmental control of ontogeny, the principle of natural selection that of phylogeny. The arguments invoked to support Skinner's and Darwin's common claims appear to be fundamentally similar. Additional evidence of similarity are Darwin's frequent remarks on the special difficulties in understanding natural selection encountered by those trained in mathematics and physical science. In pointing to limitations in "the causality of classical mechanics," Skinner has isolated the locus of the problem.

Skinner and Darwin are also alike in provoking fundamentally identical counterarguments from their critics. Leaving aside those criticisms that could only have arisen from failure to read the original writings – and this is a substantial portion of the lot – both Darwin and Skinner have been charged with asserting just about every absurdity that they did not specifically deny. As a historian of biology has observed, selectionist theory "is so easy almost anyone can misunderstand it" (Hull 1972, p. 389).

The differences among scientists regarding natural selection have been "to a large extent determined by ideological factors" and have centered upon "the fundamental scale of values" (Ellegård 1958, pp. 8, 197). So too with the reinforcement principle. Chief among Darwin's and Skinner's shared differences with nonselectionists are the attitudes toward essentialism and teleology. Skinner notes these ideological factors here, and they had been previously identified in accounts of natural selection (Mayr 1976b). The critics concur that the dispute involves basic philosophical issues, as when the approach to language by transformational generative grammarians

in systems of which human beings are subsystems, it is still interesting to pursue the functional analysis at the level of human beings. Even if, as Dawkins (1976) has put it, human beings are only "survival machines" for genes (which seems clearly spurious), this does not make a functional characterization at the level of human beings uninteresting or mistaken. (To believe so is to commit a very simple reductionist mistake. See below.)

Together these five points encourage an interest in the study of the internal structure and functioning of organisms, and a view of human beings as self-controlling. There are, I think, three ways for Skinner to avoid this conclusion.

1. He can claim that his theory of selection by consequences stands by itself without the support of Darwin's theory. But then he can hardly defend the exclusive supremacy of his choice of metaphor. If it is in the "nature of scientific inquiry" in biology that human beings are self-controlling, his case will be lost.

2. He can rely on reductionism and argue that since organisms and their behavioral repertoires can be studied in the process of evolution, we should study this process rather than the organisms. There are hints in this direction in "Consequences": "It is true that all species, persons, and cultures are highly organized, but no principle of organization explains their being so. Both the organization and the effects attributed to it can be traced to the respective contingencies of selection." But so what? The theory of evolution is a theory of change, and it helps us understand the origin of the characters of organisms, but this does not mean that our study of these characters is best pursued in terms of their origin. Nor does Skinner really want to be committed to such an extreme version of reductionism. Observing that human behavior is ultimately "all a matter of natural selection" he still wants to pursue psychology and anthropology as separate disciplines rather than reduce everything to biology. And his argument for this move reveals his understanding of the business of science ("operant conditioning, occurs at a speed at which it can be observed from moment to moment"), namely, to produce results. But the same argument can be directed against Skinner's impossible dream of gaining complete control, both theoretically and practically, of the environment.

3. Finally, Skinner can accept that human beings are self-controlling systems, but claim that this does not make them "initiating agents." He has left the notion of "initiating agent" so vague as to make this move possible. But such a move seriously limits the force of his position, making it no longer incompatible with the current paradigm of cognitive psychology.

The Darwinian solution to the problems facing our species amounts to increasing the knowledge we as human beings have of our place in nature so that we can increase the level of self-control of our interaction with nature. To increase our self-control means to increase our liberty. The Skinnerian solution is radically different. Skinner's program for education and social reform delegates no control to human beings, except to the cadre of educational officers working on how to control us (and themselves) by controlling our environment. My point is only that this program, with its views of human beings, is in no way supported by the Darwinian theory of natural selection.

Replicators, consequences, and displacement activities

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I find Skinner's article "Consequences" admirable. Selection by consequences is a good phrase, which puts a correct emphasis on the radical difference between active selection by a choosing agent on the one hand, and the blind – I almost said inconsequential! – mechanical purposelessness of the Darwinian and quasi-Darwinian processes that Skinner lists, on the other. Konrad Lorenz (1966) reached similar conclusions about the relationship between natural selection and reinforcement learning. But I believe it is important to be even clearer than Skinner and Lorenz were about *exactly what* the entities being selected are, and exactly how they are to be distinguished from their consequences. The entities that are selected, at whatever level, must be "replicators," entities capable of forming lineages of duplicates of themselves in some medium. At Skinner's level i, the ordinary Darwinian level, the replicators are genes, and the consequences by which they are selected are their phenotypic effects, that is, mostly their effects on the embryonic development of the bodies in which they sit. A gene affecting running speed in an antelope, for instance, survives or fails to survive in the form of copies down the generations, by virtue of those consequences on running speed. Genes whose consequence is a slow gait tend to end up in predators' stomachs rather than in the next generation of antelopes. Individual organisms are *not* replicators: They are highly integrated bundles of consequences (Dawkins 1982).

A case can be made for generalizing the idea of consequences to "extended phenotypes," to consequences of a gene upon the world outside, for instance consequences of a beaver gene upon dam size and hence lake size. Such consequences could be important for the survival of the gene itself. Be that as it may, the important point is that the distinction between "that which is selected" (the gene) and "the consequences by which it is selected" (phenotypic effects) is stark and clear, and is made particularly so by the central dogma: There are causal arrows leading from genes to phenotypes but *not* the other way around (the other way around would constitute the well-known Lamarckian heresy). I would like to know whether the equivalent of the central dogma holds at Skinner's other levels

At Skinner's level ii the replicators are habits in the animal's repertoire, originally spontaneously produced (the equivalent of mutation). The consequences are reinforcement, positive or negative. The habits can be seen as replicators because their frequency of emergence from the animal's motor system increases, or decreases, as a result of their reinforcement consequences. Note that, in their role as replicators, if habits are analogous to anything in the Darwinian scheme, it is to genes, not to individual organisms. But they are clearly not very close analogues of genes, and this makes the whole application of the Darwinian analogy at this level difficult.

Something like level ii selection by consequences can go on in imagination – simulation in the brain. The animal sets up a simulation in its head of the various actions that it might pursue and, importantly, their probable consequences. The simulated consequences feed back and influence the choice of action. The process can easily be described in subjective shorthand – "If I do P, I can see that the consequence would be X, so I had better do Q instead" – but there is nothing mystical or necessarily conscious about it: It goes on in electronic computers all the time, and the computer programs that do it are not necessarily very complex, although the more interesting ones are. This process should probably not be lumped under level ii, but given its own level ii-½.

I have a misgiving about Skinner's level iii, the cultural level. This is not because, as we shall probably be repetitiously told by other commentators, he is "reductionist" (whatever in the world that may mean), but because he is insufficiently clear about exactly what the entities are that are being selected, and what the consequences are by which they are selected. Is it the cultural practices themselves that replicate, that survive or fail to survive in the milieu of a single society in virtue of their consequences (Cloak 1975)? An example of this might be a hit tune that survives in the milieu of American society in virtue of its catchiness. Here the tune (or, more strictly, the representation of the tune in people's brains) is the replicator; it replicates itself when its engram is duplicated into a new brain, when a

Skinner considers operant learning a microcosm of the other two phenomena, I consider it first.

In operant learning selection is judged by a change in probability of responding that occurs when an environmental contingency links responses and outcomes. Though this statement seems simple enough, there are critical qualifications on the nature of contingencies and outcomes. Selection of a particular response occurs when: (1) there is a contingency relation that produces temporal and spatial conditions that support the development of a representation of the relation among environment, behavior, and outcome (such a representation need not be "cognitive" or complete, but it must be present in some form; without the linkage provided by this representation there is no consequence); (2) the contingency relation involving a particular response produces a stronger representation than any other one related to the same outcome; (3) the outcome is a reinforcer (that is, an event or circumstance capable of producing learned changes in responding); and (4) the reinforcer is the most important one available at that time. What constitutes a reinforcer has been a point of contention, and Skinner initially settled for a definition in terms of its effect, later arguing for a basis in natural selection. However, it appears that a reinforcer is most reasonably seen not as an event, but as a circumstance produced by a challenge imposed by the contingency on the regulatory systems underlying behavior (e.g. Hanson & Timberlake 1983).

Not only are contingencies and outcomes subject to qualifications, but the nature of selection also has particular qualities. First, the level of responding under a contingency is functionally related to characteristics of the outcome (e.g. quality of reward), and the relation prescribed by the contingency (e.g. fixed ratio schedule). Second, selection is conditional in that it applies primarily within a particular stimulus setting. Third, selection can be reversed (at least partially) by omitting the outcome.

In short, the key elements in selection by consequences in operant learning are: (1) an environmentally based linkage between behavior and outcome that supports the development of a representation of the relation among the specific stimulus environment, the behavior to be changed, and the outcome; (2) an outcome (circumstance) that contributes to this linkage and motivates its expression in performance; (3) a comparator that determines which of the available behaviors and linkages to pursue; and (4) the possibility of removing the environmental linkage and reversing the selection effect.

In biology, selection occurs as alterations of the gene pool when genetic or environmental change results in differential survival of individuals. Presuming that it is possible to treat the gene pool as analogous to the potential repertoire of individual behaviors, there are still major problems in applying the concept of selection by consequences. The most fundamental difficulty is that the contingency or link between selection and outcome is not defined before the fact. The absence of an a priori linkage means there are no characteristic relations between the gene pool and survival that produce selection; in the language of operant conditioning, one cannot specify reinforcing circumstances, or relations between reinforcing circumstances and changes. Even after the fact of survival is established, the changes in the gene pool are complex. Genes both relevant and irrelevant to survival will meet a common fate because they are grouped by individuals. This is a fundamental fact in biology, and only a side issue in the case of learning and behavior. Essentially, the absence of linkage means there are no specifiable consequences, just events that change the gene pool by eliminating some of it. This removal is neither conditional in the sense of occurring only for particular stimulus conditions, nor reversible.

Further, there is no integration of possibilities of action in natural selection. If conditions are generally appropriate for the gene pool to be depleted in two ways, it will be depleted in two ways. In learning, if conditions are generally appropriate for behavior to be altered in two ways, it is more likely that only one way will occur. This need not imply a homunculus decision maker, but simply a mechanism for integrating possible behaviors that compete for a final common path. Finally, although evolutionary change has a functional "memory" of past successes in terms of the elements available in the gene pool, this memory is quite different from what occurs in learning. In learning, the animal learns not to emit a particular unsuccessful response in particular stimulus circumstances, but the response is still available in other situations. In evolution, if genes of a type that have been selected against are gone from the pool, they are not available in any situation, and if a few examples remain in the pool, they will be expressed independently of their failure to produce survival in a particular circumstance the last time.

In short, in natural selection there is no a priori environmental linkage of gene pool and survival and thus no representation of the relation among particular stimulus environments, genes, and survival. Since consequences as used in operant learning require an environmentally defined linkage, it follows that natural selection is simply selection, not selection by consequences. Changes that occur in the gene pool are not conditional, reversible, or functionally related to characteristics of the outcome or the contingency. Outcomes are not remembered conditionally. Furthermore, much of a given change in the gene pool is completely unrelated to the particular circumstances of individual death or survival. Finally, there is no momentary integration of possibilities in terms of efficiency or importance.

In the case of culture, Skinner's focus varies between social reinforcers of individual behavior and the role of selection by consequences in the survival of cultures; however, the last seems to be the most important to his argument. Skinner treats cultures as combinations of elements in a sort of cultural gene pool. Changes in the pool occur as a consequence of differential survival of cultures. Most of the objections raised to viewing natural selection as selection by consequences apply here as well. Again there are no consequences, just effects. Skinner seems to see that in terms of selection by consequences the interesting phenomenon is the way in which individuals contribute to culture through learning and innovation, but this interaction is more illustrated than analyzed.

Skinner has contributed uniquely to the continuing struggle to develop models of learning and behavior largely free from stultifying concerns with imaginary causal agents. I believe he is right in his concern that we not slip back into inventing causal concepts that depend almost exclusively on our private models of how we behave. However, in the present case I think his concern with general mechanisms has led him to ignore critical differences among phenomena. He has generated parallels among natural selection, learning, and cultural selection that, although initially thought provoking, are without much long-term heuristic value. His insistence on a common causal mode has not promoted a more complete analysis of these phenomena or their relations. In some ways this work is a mirror image of recent sociobiological explanations of behavior. Sociobiologists attempt to explain everything at the level of gene survival; Skinner attempts to explain everything at the level of a common general mechanism. What is needed at present is an approach that captures, expands, and integrates these levels of explanation.

The beginning of an integrative approach lies in the assumption of evolutionary biology that all behavior, including learning and elements of culture, is based on the differential survival of genes promoting these phenomena. However, the local basis for an integrative approach must be in terms of local mechanisms. An analogy may be useful in clarifying the situation. Suppose that we have a large computer that has the single function of

assembling chess-playing programs from a pool of program elements. (The assembly actually takes place probabilistically by sampling without replacement from smaller subpools of the program elements, so that individual programs may have similar elements and all programs are complete.) Periodically the computer puts together a new group of programs and sends them through a series of tournaments before returning the elements to the pool. The elements of the most successful program are doubled, and the elements of the least successful program are removed. This process is roughly analogous to natural selection. There is no a priori linkage between particular aspects of the gene pool and survival, and there is no conditional memory for success or failure. Given that the elements remain in the pool, the same unsuccessful program can be assembled again.

To add learning by consequence we must allow the programs to profit from experience with local successes and failures in each game. To facilitate learning it will be useful to provide a representation of the relation between stimulus conditions, behavior, and outcome, to evaluate the importance of any outcome in the context of the game, and to allow the representation to be conditional and reversible. Finally, to add culture we must allow some of the programs to have access to past relations between behavior and outcomes compiled by previous programs of a similar sort. Competition between cultures would be based on survival of particular kinds of elements in the pool. A simulation of such a system might provide further insight into the relations among the levels and effects of biology, individual learning, and culture in determining behavior.

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Giving up the ghost

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We may construe the history of science in part as consisting of three major revolutions which have radically altered man's view of himself. Prior to the Copernican revolution, the fusion of Christianity and Aristotle taught that the earth was the center of the universe, that we had been created in the image of an omnipotent and omniscient being, and that, governed by reason, we could act to save our immortal souls. The work of Copernicus led inexorably to the view we now hold of the universe: Our sun is a star of a fairly common variety, situated about two-thirds of the way from the center of one of many spiral galaxies; our local group of galaxies is in turn part of a larger grouping, the Virgo cluster. The universe at large is indifferent to mankind.

Darwin ushered in the second revolution, arguing that we were not created by an omniscient being. Rather, by means of the joint action of variation and selection, plants and animals had, over millions of years, gradually become better adapted to their environments. We were the product of that adaptation. Dawkins (1976) gives us a contemporary picture of where Darwin's position led. Constructed by genes, we are machines that tend to act in such a manner that more of the genes that created us are in turn created. Other organisms within our environment are not indifferent to us, but rather act to bend us to their aims.

Selection by consequences is one step in the third, Skinnerian, revolution, which will have far-reaching implications regarding our very identities. Previous revolutions left basically intact our assumptions that we are moral, responsible beings, free to choose a course of action, justly punished if we make the wrong choice. The main thrust of Skinner's position, exemplified in "Consequences," is that voluntary behavior is not. Various forms of behavior arise, some are strengthened and some not, and we are left with some subsequent distribution of behavior.

The parallel between evolution and behavior allows the derivation of a number of strong conclusions. Ernst Mayr (1976a, p. 28) contrasted the view that a species consists of a fixed type with the more scientific view that a species consists of a distribution of organisms:

> The ultimate conclusion of the population thinker and of the typologist are precisely the opposite. For the typologist, the type (eidos) is real and the variation an illusion, while for the populationist the type (average) is an abstraction and only the variation is real. No two ways of looking at nature could be more different.

Following up the parallel with regard to human behavior could lead to a profound change in our views of individuals. What we call our identity may more properly be described as an average form of behavior maintained by a relatively constant environment. Although it may be painful to give up the position that each of us is, or has, an integrated self or identity, in the long run the closer we are to the true state of affairs the better off we will be. [See also Ghiselin: "Categories, Life and Thinking" *BBS* 4(2) 1981.]

Natural selection and operant behavior

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Skinner's article "Consequences" offers the reader a fascinating intellectual adventure. In a few pages which contain enough material for several large volumes, the author presents his complete concept of the origin and maintenance of behavior of animals including, specifically, humans. The "selection by consequences," as Skinner calls his concept, operates on three levels: natural selection, individual behavior, and evolution of cultures. As the phenomena of at least two of these levels cannot be directly or easily observed, the concept must, nolens volens, be based on suppositic s and simplifications. This gives the reader an opportunity to ask questions. Here are some of them. Does the development of behavior really resemble the process of natural selection? Are the consequences in each case of the same nature?

Let me concentrate first on natural selection. According to Darwin's theory, only those individuals and species survive that have a genetic ability to cope with the changing environmental conditions. This results in the development of new variations and new species, in other words, in evolution by natural selection, a process characterized by Skinner as "selection by consequences." This process, completely passive, occurs without any interaction from the individual organism. But the inborn features critical to survival are transferred to the next generations.

The evolution of the animal world does not seem to be a straight-line process. Insects, for instance, are admittedly much lower in the evolutionary hierarchy than vertebrates, but their central nervous system, although different from that of vertebrates, is quite extensive, and their motor abilities (relative to the size of the body) and the sensitivity of certain sensory systems (such as olfaction), at least in some species (e.g. ants), surpass those in vertebrates. And, among vertebrates, birds (for instance) have better vision than mammals. Birds also have a differently developed muscular system. The pectoral muscles supporting the action of the wings are enormous compared to corresponding muscles in mammals. Humans who consider themselves as being at the top of the evolutionary scale, not only

have poorer vision than birds, but also a poorer muscular system than other mammals of similar size. In fact, humans would not be able to defend themselves against predators if they were not equipped with more efficient brains. The development of the large cerebral hemispheres with their associative cortex compensated for the deficiencies of other systems of the body, and not only resulted in the survival of humans but also secured them the highest position among living beings.

Let me turn now to individual behavior consisting, for the most part, of operant behavior (using Skinner's terminology). Does the development of operant behavior in an individual resemble the evolution of life over millions of years? It has been asserted that the successive stages of ontogenetic development roughly approximate the successive stages of phylogenetic development (Gould 1977). This process occurs mostly during the prenatal period and continues for some limited time after birth. Then operant behavior starts to develop in response to environmental conditions.

The development of operant behavior and the evolutionary process are similar in that they are both based on selection by consequences and both become gradually more and more complex. But there are also big differences between them. By contrast with natural selection, (1) operant behavior is an active process capable of producing permanent changes in inborn reactions and complicating their patterns; (2) its development seems to be quite a straight-line process unless slowed by adverse conditions (including aging); and (3) it is not genetically transferable to later generations.

But there is still another important difference between natural selection and operant behavior. Although in both cases selection by consequences is the basic principle, the consequences are different in each case. In natural selection the consequence is survival. But is it also that in operant behavior? Let us take, for instance, feeding behavior. It has been reported that 6–7-day-old rat pups prefer nonnutritional 0.1% saccharin solution in water to 2.8% lactose solution corresponding to the sweetness of mother's milk (Jacobs & Sharma 1969). Adult rats, even when hungry, prefer nonnutritive 0.25% saccharin solution to nutritive 3% glucose solution (Valenstein 1967). Other experiments have shown that hungry rats, offered a choice between food and intracranial self-stimulation, prefer to self-stimulate, although this leads to death from starvation (Routtenberg & Lindy 1965). Excellent examples of behavior contrary to survival are also provided by drug addiction and dangerous sports. It seems, then, that the consequences of operant behavior must be not so much survival as sensory gratification. It can be supposed that what is called "reinforcement" in operant behavior is sensory satisfaction or, in other words, improvement in sensory state resulting from the presence of unconditioned stimuli in approach behavior, or from the absence of unconditioned stimuli in avoidance behavior (Wyrwicka 1975; 1980).

So far, there is no objective and direct evidence that improvement in sensory state is the main causal factor in operant behavior. Still, can we be sure that animals living in their natural environment do not care about the taste of food and eat only in order to survive? Or that they mate only in order to produce progeny (that way securing the survival of the species), and not in order to get sensory satisfaction from mating? Of course, there exist behaviors where survival is in stake. These include fights with competitors for territory, food, or mates. But is survival the real "purpose" of the fight? It may be so, but on condition that survival means experiencing sensory gratification.

If the above supposition is correct, this means that the survival of the species is secured only when sensory satisfaction obtained from operant behavior goes together with survival. On the other hand, in cases in which it works against survival, the whole species can perish. This especially applies to humans, who have developed such a variety of means to provide sensory satisfaction.

Author's Response

Some consequences of selection

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Why has the role of selection by consequences appeared so late in the history of human thought? It is a principle found only in living things and that is no doubt relevant, but people have been interested in living things as long as in nonliving. A more likely explanation is that the effects of selection are somewhat delayed. We see the products of selection but only some time after we have seen the selection itself. The difference in time may have led to the search for current surrogates. We look for operative features in the product rather than at the selective events responsible for them.

Purpose is such a current surrogate of past consequences. Cognitive psychologists speak of operant behavior as goal-directed behavior. Goal-directedness is a current property which replaces a history of reinforcing consequences. (The word operant alludes to an observed property of behavior – namely, the effect on the environment. Whether the effect changes behavior is not mentioned.) The intentionalism of modern philosophy also springs from a search for a current property of behavior as a surrogate for history.

The temporal subtleties of selection could in turn be responsible for the invention and subsequent flourishing development of a simpler cause, following the push-push causality of daily life. Thus, life on earth has simply been created and behavior is simply intended, chosen, and willed.

The reasons why selection by consequences was so long neglected are probably the reasons why it is still so badly misunderstood.

I am sorry that in the four or five thousand words that were available to me when I wrote "Consequences" I have not covered the field of natural selection to **Barlow's** satisfaction. I am also sorry that he appears not to be aware of the extent of current research on operant conditioning. I am happy that he agrees with me on the evolution of culture, but he seems to miss its relevance to the question of who is to decide what is good behavior.

So far as the point of "Consequences" is concerned, it does not matter in the least whether any of the behavior Barlow mentions is the product of natural selection, operant conditioning, the evolution of cultural practices, or any combination thereof. The same issues arise: the need to abandon the concept of a creator, purpose, essences like life, mind, and zeitgeist as contemporary surrogates of histories of selection, and values. I repeat: All these issues demand attention regardless of whether the consequences are found in natural selection, operant conditioning, or the evolution of cultures. Although there is lively controversy at all three levels, the basic notion of selection by consequences survives and raises the questions I addressed.

Bolles gives 1959 as the date of the first observation of similarities between operant conditioning and natural selection, but in 1953 in *Science and Human Behavior* (p. 430) I wrote: "We have seen that in certain respects operant reinforcement resembles the natural selection of evolutionary theory. Just as genetic characteristics which arise as mutations are selected or discarded by their consequences, so novel forms of behavior are selected or discarded through reinforcement." And I went on to say that "[t]here is still a third kind of selection which applies to cultural practices . . . [a] practice modifies the behavior of members of the group. The resulting behavior may affect the success of the group in competition with other groups or with the nonsocial environment."

In his last paragraph, Bolles brings up an interesting point. As an explanatory mode, selection is responsible only for novelty, for origins. That is the way in which it differs from the causal mode of physics. Once a given structure has been selected by natural selection and once a bit of behavior has been shaped by operant reinforcement, selection as a causal mode has done its work and a mechanical model may suffice. A survey of the current state of the organism – the responses in its repertoire, the relevant reinforcing consequences, the controlling stimuli – need not involve selection at all. Nor will the neurological account of how these variables are interrelated. Only if these structures are still changing will selection need to be considered as a causal mode. So far as they are the products of selection, a "mechanical" causality suffices.

Boulding has offered an image to be corrected "so that knowledge becomes more perfect." A few corrections:

1. There are many fields which now lie beyond prediction and control. Evolution is one, plate tectonics another, and astronomy beyond the solar system a third. Do we remain silent about them? No, we interpret observations in those fields by using what we have learned from research in which we *can* predict and control. Most educated people accept such interpretations in lieu of the explanations which have come down to us from folk culture and religion. Human behavior is such a field, and I am confident that an experimental analysis has contributed much more to understanding it than Boulding says.

2. The experimental analysis of behavior is not a "black box, input–output . . . approach." (See "Terms" and "Problem Solving.") Input–output suggests a stimulus–response formulation to which operant conditioning and an emphasis on selection by consequences were correctives.

3. Reinforcers are not defined in terms of pleasure and pain. They are defined in terms of their effects in strengthening behavior. How we come to talk about them and call them pleasant and painful is mentioned in "Terms."

4. I had no space to expound evolution fully. But I *did* explain the transmission of learned structures from one generation to the next in the discussion of imitation and related topics.

5. Operant conditioning is not quasi-mechanical. It is, as I point out in my paper, the clearest evidence we have of the process of selection by consequences. As I say in my reply to **Bolles**, selection is concerned with origins; once a system has come into existence it can be studied in other ways. I would not look for much help, however, from processes in the nervous system by which images are coded.

Unlike **Campbell** I believe it is correct to classify "a child's ball, the planet earth, and an orange as spheroids" and conceivably useful to do so in raising the question of why things so diverse are nevertheless roughly spherical. I think it is useful to point out that the four issues I raise – origination, purpose, essences, and values – are due to the nature of selection as such and not to any particular variations or selected consequences common to the three levels.

Campbell seems to feel that all selection must be due to genetic change. Thus, in discussing operant behavior he says that "behaviors might become shaped in such a way as to be appropriate for novel environments *and lead to differential reproduction*. For this to occur these behaviors or their possessors would have to be acted upon by natural selection" (my italics). But the italicized phrase had nothing to do with operant conditioning. The *process* has presumably evolved because it led to differential reproduction, but it operates through consequences of its own.

I have no objection to the definition of selection pressure that Campbell cites. My objection is not that it is "an attempt to assimilate selection to the causality of classical mechanics" but merely that, as Campbell says, "the term 'pressure' is too reminiscent of physics." By saying that selection pressure is not necessarily exerted by other species, I meant merely to defend evolution against Social Darwinism. Such an idea may not be common among biologists, but it has been vigorously discussed.

I believe that operant conditioning supplements natural selection, but I did not suggest that it could replace it completely. A far greater fraction of the behavior of a species like Homo Sapiens is due to operant conditioning than is, say, that of an insect. The human species has shown a much greater capacity to adjust to novel environments by turning to operant conditioning as the principal source of its behavior.

I did not, as **Dahlbom** implies, choose "the metaphor of natural 'selection'" to describe operant conditioning. I had done research on the selection of behavior by consequences for many years before the similarity to natural selection suggested itself. Selection is not a metaphor, model, or concept; it is a fact. Arrange a particular kind of consequence, and behavior changes. Introduce new consequences, and new behavior will appear and survive or disappear. Individuals gain the "flexibility" that Dahlbom regards as essential precisely from the fact that their behavior is modified by consequences in their lifetime rather than through natural selection. Thus, I can claim that my "theory of selection by consequences" stands by itself without the support of Darwin's theory, and there is no "exclusive supremacy of [my] choice of metaphor" to defend.

Dahlbom may be surprised to learn that my *Science and Human Behavior* (1953) is said to be the first text in psychology to have a chapter on self-control. We do control ourselves, but not as initiating agents. We control ourselves as we control the behavior of others (by chang-

ing our environment), but we do so because we have been exposed to contingencies arranged by the social environment we call our culture.

Organisms avoid self-destructive behavior without foreseeing the consequences, but, as I explain in "Problem Solving," people no doubt do so more effectively when they have analyzed the contingencies and, in that sense, have foreseen the consequences.

I agree that any dream of gaining complete control of the environment is "impossible," but from what we learn when the environment is reasonably well controlled, we can at least interpret what is happening under more chaotic conditions.

I thank **Dawkins** for his refreshingly helpful commentary and confine my remarks to questions he asks about levels ii and iii.

I do not know whether an "animal sets up a simulation in its head of the various actions that it might pursue and, importantly, their probable consequences," but people do something much like that when they examine prevailing contingencies and construct rules to be followed to respond to them effectively (see "Problem Solving"). Dawkins's suggestion that displacement activities at level ii may have the effect of mutations at level i throws light on creativity – another chestnut in evolutionary theory closely related to this paper. Creative artists know how to create mutations from which they then select those that are beautiful in the sense of reinforcing to them, greatly increasing the chances that their work will be original. My only trouble with Dawkins's suggestion is that displacement activities tend to be stereotyped, but – who knows? – mutations may be, too.

There is clearly a question about what exactly is being selected and what are the selecting consequences. Within a given group, the answer seems to be practices – better ways of hunting, gathering, growing, making tools, and so on. The practices are transmitted from generation to generation when those who acquire them under the contingencies arranged by one generation become the transmitters for the next. There is no competition between cultures, no Social Darwinism, in such a formulation. But cultures as a whole have also come into existence and perished. As I point out in my replies to **Harris** and **Maynard Smith,** the evolution of cultural practices is like the evolution of heart, stomach, eye, ear, fin, leg, wing, and so on. The evolution of cultures is like the evolution of species, each of which may have a particular kind of heart, stomach . . . , and so on. It is clear that cultural *practices* do not evolve because of successful competition between cultures, except where the practices have to do with conflicts between cultures – for example, the invention of more powerful weapons. A culture which strengthens itself by developing new methods of agriculture, new social systems, and so on is more likely to compete successfully with another culture, but the practices themselves evolve because of contributions to the group that would also prevail if there were no competition with other groups.

Donahue wonders whether cultural evolution, or the evolution of cultural practices, is a different *kind* of selection. I think it is, although I see in it no new behavioral process. I think operant conditioning explains the discovery of new practices and their transmission to other (especially younger) members of a group. But one may still identify variations (new practices), reproduction (the transmission to others), and selection through consequences, whether for the individual or the group (and that last phrase distinguishes between the evolution of cultural practices and the evolution of cultures). For example, the use of a new food or a new way of planting or storing it will be transmitted to other members of a group because of its reinforcing consequences for individuals, who are thereby more likely to escape hunger. The group may then compete more successfully with another group (say, for available land). In neither case is a special genetic trait at work, as in kin selection. The evolved process of operant conditioning is a sufficient explanation.

I do not agree that respondent and operant conditioning are best regarded as "simply different *procedures* for studying behavioral change." As Ferster and I pointed out in *Schedules of Reinforcement* (Ferster & Skinner 1957), a term like "conditioning" or "extinction" is traditionally used to refer to two very different things: (1) the role of the experimenter or the environment in bringing about a change, and (2) the resulting change in the organism. Donahue seems to add a third, "*procedures* for studying behavioral change." We are concerned here with behavioral processes as they must have existed before anyone promoted them or studied them. Whether there is a neurological principle common to respondent and operant conditioning is a question that will presumably be answered by neurologists; the two types of conditioning are still clearly distinguished by the contingencies under which they occur.

I certainly do not claim, as **Gamble** implies, that experimental behavior analysts were the first to suggest a parallel between Darwinian selection and "trial and error learning," but I contend that the experimental analysis of behavior is by far the most detailed examination of the contingencies of selection responsible for the behavior of the individual. I also believe that selection at level iii does not require a process different from natural selection or operant conditioning. I certainly did not mean to suggest that very much has been done in those fields by operant conditioners. Indeed, I regret that more has not been done.

I do not see the relevance of Gamble's comments on one-trial learning. As I showed more than 50 years ago (Skinner 1932), an operant like pressing a lever is easily conditioned by one reinforcement. I do not suppose Gamble means that a complex bit of phylogenic behavior (say, building a nest) once occurred in that form as a variation and was selected by its consequences. It must have been the end result of a long process of shaping. I have reviewed a few established geological processes which could have supplied conditions for a gradual approach to complex phylogenic behavior (Skinner 1975).

I do not treat culture, as **Ghiselin** claims, "as if it were identical with verbal behavior." I said that verbal behavior (which I had just discussed) greatly increased the importance of a third kind of selection by consequences. Other ways in which new forms of behavior are transmitted to new members of a group include imitation and modeling. I would define a culture as "a mass noun" as a

ocial environment. Children are born into a culture imply in the sense that their behavior will be shaped and naintained by contingencies of reinforcement in which other people play a part. If a group of people is confined to one locality, its physical features may also be included as part of a culture. A child's behavior is the result of both.

Features of a social environment (separate cultural practices as variations) come into existence for many reasons which need not be related to their effects upon members of a group. They are transmitted to new members when the members learn either by imitation without modeling or by explicit modeling, or through advice, warnings, maxims, rules, laws, and other verbal devices, and when those who have thus been changed become in turn those who compose the social environments of others.

Ghiselin reports that those who have been working on adaptation have "learned not to ask what is good for the species or anything else." In the experimental analysis of behavior, a specific example concerns what is reinforcing. I might paraphrase Ghiselin by saying "Right thinking means asking, not what is reinforcing, but what has happened." We *discover* what is reinforcing to an organism; we do not predict it. Things do not reinforce because they are good or feel good. I believe the same point can be made for natural selection and the evolution of cultures; I discussed the issue briefly in "Consequences" under the heading "Certain Definitions of Good and Value."

I am not sure that Ghiselin is characterizing my position as "the automaton theory of behavior," but automaton suggests the classical mechanical causal mode, which I am suggesting is not applicable. I cannot say "how far the genes possess hegemony over the intellect," but in "Consequences" I said, "Ultimately, of course, it is all a matter of natural selection, since operant conditioning is an evolved process, of which cultural practices are special applications." I do not believe that there is something called "intellect" or "thoughts" which belongs in a different world.

Hallpike lists his objections in very strong terms. I have "nothing to say about" *A*; my model *B* "is totally inadequate"; *C* is "sheer mystification." I fail to grasp that *D* . . . ; there is "apparent emptiness" in *E*; *F* is a "fundamental defect"; an important point *G* is ignored. *H* is "profoundly incorrect, because it totally ignores . . ." Yet I do not see any great difference between us except in Hallpike's understanding of what I have written.

"*Something* has first to come into existence before it can be 'selected.'" Of course. It is an old problem in operant behavior: A response must occur before it can be reinforced. Cultural practices no doubt have many kinds of origins. Some may be accidental, some may be designed ("conscious?"). Design may take selective consequences into account (see "Problem Solving") but even so may be random with respect to the evolution of the practice. Both accidental and designed practices are effective first in reinforcing people. They become practices only when they are transmitted as parts of a social environment. I do not know how metal tools were discovered, but the advantages Hallpike singles out all have to do with contingencies reinforcing the behavior of an individual. I can easily compose a scenario in which the smelting and use of metals came into existence through entirely accidental contingencies, and I do not think the account would be mystification.

I do not see that the analogy between social and biological evolution breaks down because of "dialectical interaction between innovation and society." Certainly changes due to natural selection alter the contingencies for further selection.

A current problem in evolutionary theory has to do with the fact that some species do not change during very long periods of time. I thought it worthwhile to mention the parallel in human cultures, but Hallpike's summary ("In short, either people go on doing what they have always done, or they do not, and innovations may occur in existing circumstances or in new ones!") is "emptier" than what I said. If cultures do not change, it is either because new variations have not appeared or because those which have appeared have not been selected for by the prevailing contingencies.

I can understand why Hallpike may regard my neglect of structure or organization as a fundamental defect, since he is apparently a structural anthropologist for whom those are fighting words. But I should want to underline his admission that "of course structure or organization cannot by itself *do* anything." Hallpike's solution – that although only real, individual people have any real causal powers in space and time, the individual members of a society are not causally autonomous – is my own. They are not causally autonomous because their behavior is controlled by a social environment. (Naturally "even" I would "shrink from 'explaining' slavery by claiming that owning slaves is reinforcing for the masters, and that submitting to slavery is reinforcing for the slaves;" The word "slavery" suggests different contingencies.)

Of course I do not say that societies are "real entities with goals and needs of their own distinct from those of their members" except in the sense that a social environment is distinct from the individuals whom it affects.

Hallpike misunderstands my point that social behavior is within easy reach of natural selection because other members are among the most stable features of the environment of the species. There is a reason why so much of the behavior studied by ethologists emphasizes courtship, mating, nest building, and the care of young. In addition to their obvious relevance to individual survival, these classes of behavior could evolve because mates and offspring are necessarily constant parts of the environment – unlike, for example, a particular food supply or nesting material, where phylogenic behavior has a lesser chance to evolve. To call that point profoundly incorrect because it totally ignores the dialectical interaction between individual behavior and sociocultural structure is putting it rather strongly.

I am glad that Hallpike does not believe that societies were created by wild men emerging from the forest and shaking hands. I did not say that selection by consequences is superior to *all* other theories, but I do wish that it were true that "theories of group minds and zeitgeists [had] long gone the way of the Absolute as serious subjects for debate in social evolution."

As **Harris** points out, I defined "a third type of selection by consequences" as "the evolution of social environments or cultures." The examples I gave, however, –

better ways of "making a tool, growing food, or teaching a child" – are far from the "extinctions of regional or local repertories caused by war, famine, and other catastrophes," which sounds more like Social Darwinism. I said quite explicitly that, as Harris insists, the first effect occurs "at the level of the individual," but there is another effect which can be stated only at the level of the group in spite of the fact that it is always an individual who behaves. If the evolution of a culture could be said to correspond to the evolution of a species, then the evolution of cultural practices corresponds to the evolution of eyes and ears and hearts and legs and wings.

It is not hard to define a cultural practice, but what is a culture? It is more than a group in the sense of the inhabitants of a given place. To speak of their common values is simply to appeal to common selective contingencies, as I note in "Consequences." I have taken a culture to be a social environment, the contingencies of reinforcement maintained by a group which, in addition to the physical environment, are responsible for the repertoires of new members of the group. Harris puts it this way: "Human behavioral repertories consist overwhelmingly of operantly conditioned responses that are at the same time culturally conditioned responses, that is, responses shaped in conformity with culturally determined reinforcement schedules and contingencies." But a culture is *transmitted* (and the mode of transmission is at the heart of selection) when individuals who have been changed by the contingencies maintained by a group become part of a maintaining group. That process requires operant conditioning, but it is a different contingency of selection.

I did *not* say that contingencies of selection occur *merely* in "documents, artifacts, and other products of . . . behavior." I was speaking of the metaphor of the storage of contingencies of selection in genes and the nervous system, and I said that the social environment could be regarded as an exception because "*parts* of [it were physically] stored in documents, artifacts, and other products of that behavior" (italics added).

When Harris writes: "Behaviorist principles can tell us how these individuals shape each other's behavior, but they cannot tell us what behavior they will shape," I would put it this way: Individuals shape each other's behavior by arranging contingencies of reinforcement, and what contingencies they arrange and hence what behavior they shape are determined by the evolving social environment, or culture, responsible for their behavior.

I have not read Campbell's 1960 paper and, as **Honig** says, it may well have anticipated the argument of "Selection by Consequences." But I had already made my point in *Science and Human Behavior*, published in 1953. As I said in my reply to **Bolles**, I wrote on page 430,

> We have seen that in certain respects operant reinforcement resembles the natural selection of evolutionary theory. Just as genetic characteristics arise as mutations and are selected or discarded by their consequences, so novel forms of behavior are selected or discarded through reinforcement. There is still a third kind of selection which applies to cultural practices.

A rather elaborate analysis of survival value and its relation to other kinds of value then follows.

The Breland and Breland (1961) report is now more than 20 years old and, in spite of the attention it aroused, it has not, so far as I know, been analyzed in controlled scientific research. Most of the instances reported were not examples of a "'drift' toward the consummatory response required by the reinforcer that follows the response" but a sudden intrusion of phylogenic behavior. I myself did experiments on what Jenkins and Moore (1973) called autoshaping as early as 1946 and referred to it in my notes as the classical conditioning of a stimulus eliciting an exploratory response (Skinner 1983a; 1983b, p. 134). If Honig is using the word "anticipatory" in its dictionary sense, it is a problem to explain rather than an explanatory principle. How can an anticipated event affect behavior?

Katz gives a useful statement of natural selection, emphasizing that it is "brought about by mechanisms . . . entirely consistent with the well-understood laws of the physical world" yet containing something new. What is new is appropriately enough called "novelty." The key word in Darwin's title was "origin." Selection is creative, in spite of the fact that, as Katz points out, "although the complex order that is thereby created is wonderful, it does not countermand any natural laws."

Maynard Smith is right in saying that I am not interested in the structure and (physiological) development of the organism, but I believe it has a structure and that that structure develops. I simply think that structure is appropriately studied by those who possess the proper instruments and methods. I have objected only to theories of structure and development which put researchers on the wrong track. I would cite "information" as an example. I do not believe the genes "tell" the fertilized egg how to grow. Perhaps that metaphor will cause no harm, but it has caused a great deal of harm in the field of human behavior. People are changed by contingencies of reinforcement; they do not store information about them.

Maynard Smith seems to feel that the evolution of cultures must be very close to Social Darwinism. There must be many cultures, and they must compete, and some must survive and some perish. But, as I have said in reply to **Dawkins** and **Harris**, I am concerned with the evolution of cultural *practices* – with features that would correspond to heart, stomach, eye, ear, leg, fin, wing, and so on – features characteristic of many different species as cultural practices are characteristic of many different cultures. A man may invent a quicker way of making a fire because of the consequences for him. If that is imitated and transmitted, it becomes a consequence for the group and survives as such. It is the practice which survives, not the group. The practice may well contribute to the survival of the group in competition with other groups or in "competition" with the natural environment.

Plotkin & Odling-Smee nicely define their position by appealing to Piaget and, with rather more passion, to Chomsky. The question is not whether learning is doing, whether learners are doers, but whether they are initiators. Selection is a causal mode only in the sense of causing novelty – whether in the *origin* of species, the *shaping* of new operants, or the *invention* of cultural practices.

What can I reply to commentators who say that "the point . . . is to show us, not just tell us?" True, I am not a biologist or an anthropologist, but as a psychologist I have certainly published more than most of my contemporaries, and there is a very extensive literature in the experimental analysis of behavior which does *show* Plotkin & Odling-Smee what I am talking about, if they care to look. ("Methods" is one place to start.)

"Consequences" is, in a way, an answer to the three challenges **Provine** mentions. One might as well speak of biological constraints on medicine as on learning. We study the effects of drugs, surgery, and other therapeutic practices on the organisms which present themselves for treatment as we study the learning processes in the organisms in which we are interested. In neither case is anyone claiming a universal science of medicine or learning. That a given species is predisposed by its genetic history to see particular stimuli in preference to others or to behave in particular ways in preference to others are facts of the same sort. A different kind of selection has been at work.

I see nothing tautological about the definition **Rosenberg** gives of a reinforcer as "any stimulus which if presented (or withdrawn) contingent on an operant, increases (decreases) the probability of the occurrence of the operant." It is no more tautological than the definition of an *allergen*. One may guess fairly accurately that certain standard things will be reinforcers, but beyond that one must find out what is reinforcing to a particular person. The unconditioned reinforcers gain their power from phylogeny. Susceptibilities to reinforcement have had advantageous consequences and have evolved as traits.

I agree that no feature common and peculiar to reinforcers has so far been found, and I shall be surprised if one is ever found. I also agree that reinforcers have a common effect inside the body. But that is not the centrism to which I object. We commonly say that reinforcers feel good, taste good, look good, and so on, but as I suggest in "Consequences," "good" appears at all three levels as more or less synonymous with selective advantage.

I would certainly reject any "attempt to assimilate selection by consequences to the causality of classical mechanics." Selection is responsible for novelty, but as something new comes into existence the structures involved obey the laws of classical mechanics. I have not "categorically abjured" movement in the direction of studying the "states that intervene between initial reinforcement of emitted behavior and its subsequent recurrence." I have simply left that to those who have the proper instruments and practices. Introspective mentalists simply put the neurologist on the wrong track, and so I believe do cognitive psychologists. It is the function of a science of behavior at the present time to give neurologists their assignments, as it was the function of genetics prior to the discovery of DNA to give modern geneticists their assignment with respect to the gene. I look forward to a comparable development in behavior, though I do not expect to live to see it.

I found two points in **Rumbaugh's** commentary particularly interesting. Not much attention has been paid in this treatment to the possibility of intervening in selection by design. The evolution of domestic animals has been altered for centuries, and genetic engineering now appears as a much more effective discipline. We have designed individuals through the special contingencies arranged in education, therapy, and other fields, and we have proposed and tested new cultural practices. We have even altered selective contingencies to permit cultures to survive that would otherwise become extinct.

But, as Rumbaugh points out, even with these interventions the prospect of effective action with respect to the frightening problems faced by the world today is not promising. Would there be more survival value in the traditional view of man as originator and creator? Are we worsening our chances by taking a view which so many people find hard to accept – namely, that our behavior is determined by our genetic and personal histories? Two points are relevant: (1) Man the initiator, the master of his fate, has been the established view for several thousand years. Perhaps he can be given credit for the human achievement, but he is also responsible for our problems. (2) The alternative view seems to me to be promising because it points to something that is more easily changed. Rather than save the world by changing how people feel and think about it, it may be possible to create an environment in which they will acquire more effective behavior, work more productively, treat each other better, and take the future more effectively into account.

Is there a word or two missing near the end of **Schull's** first paragraph? I am puzzled by his statement that "the problem with the present scene is that it has taken [me] at [my] word and chosen dignity, purpose, and the acknowledgment of cognition over behaviorism and selection." "My word" is that doing so raises problems, and I have certainly not counseled doing so.

Cognitive science is most "fertile" in breeding promises of great achievement, such as the "disciplined analyses of cognitive functions" Schull mentions. The achievements have yet to be realized. Most of what is called cognitive science is work that was carried on in more or less the same way before that magical term was added.

I am all for *feelings* of causal adequacy as I am for feelings of freedom and dignity. I want people to be adequate, unhampered, successful, and aware of the fact that they are so, and I have suggested ways in which that may be brought about – by changing their environment.

To shift the origination of a genetic trait to something that happened in an individual is perhaps to make the individual an initiating agent for the genetic trait, but we have still to explain the origin of the behavior of the individual. We have only moved a little further along in the search for the initiating agent. As for "experimenting mentally," rehearsing, imagining, foreseeing, I am in no better position to say what is happening than anyone else, including cognitive scientists. An answer will probably come from neurology, but only in the distant future. Meanwhile, we can approach these activities without committing ourselves to any position as to their nature by looking, for example, at how we teach children to experiment mentally, to rehearse, and so on, and how to know that they are doing so.

Solomon quotes my statement that vocal responses can be modified through operant conditioning "apparently

only with respect to the occasions upon which they occur or their rate of occurrence." The statement was about species below the human level. The human species became preeminent when its vocal musculature could be much more readily modified, particularly with respect to its topography. I have not changed my position on language. Certainly no one will argue that there is an innate disposition to use a particular set of speech sounds; languages differ far too much to make that plausible. As to the universals of grammar, they are, I believe, merely the universal uses of verbal behavior by language communities. In all languages people give orders, ask questions, describe situations, and so on, and different languages work out different ways of doing so.

I have answered **Solomon**'s other criticisms in Skinner (1983a). The Garcia effect is punishment, not reinforcement, and operates precisely as I described punishment in *Science and Human Behavior* in 1953. When I said "pigeon, rat, monkey, which? It doesn't matter," I was referring to schedule performances, not to entire repertoires. I doubt that the conceptual nervous systems constructed to explain sensory, motor, and associative processes have a valuable heuristic role. Instead, they have generally led the neurologist to look for the wrong thing – for example, the supposed copies or representations which are said to be constructed in the nervous system when a person perceives a situation or remembers it later.

I am not an evolutionary biologist, but I have been at least aware of most of the issues **Stearns** brings up. Many of them have parallels in the field of operant conditioning, but a consideration would have taken far too much space. I do not think that a more accurate account of the present position on natural selection would have made much of a difference for the point of my paper, the four kinds of concepts which have usurped the role played by selection. Evolutionary theorists may not appeal to concepts like life, mind, and zeitgeist, but behavioral scientists, in the sense that includes economists, political scientists, and anthropologists, do so, and so do philosophers, theologians, and many others who have an effect on what is happening in the world today. Creation science may be easily dismissed by the evolutionary theorist, but something very much like it is a problem for the behavioral scientist. Even biologists are not free from the misuse of the concept of purpose, and the role of values is still widely debated. These are the main issues in my paper, and my lack of expertise in evolutionary theory is not, I think, a serious threat to the validity of my argument.

Timberlake's paper is in many ways a puzzle. First of all, there is its terminology. I found it hard to think of "a contingency relation that produces temporal and spatial conditions that support the development of a representation of the relation among environment, behavior, and outcome" or to see "a reinforcer . . . as a circumstance produced by a challenge imposed by the contingency on the regulatory systems underlying behavior." But more puzzling was Timberlake's apparent belief that evolution occurs because of variations in the environment rather than the organism: "Changes in evolution (new species), individual learning (new behaviors), and culture (new societies) typically occur in the context of some alteration of the environment." "In the context of" does not mean "because of," but Timberlake writes as if it did. Thus, "in operant learning selection is judged by a change in probability of responding that occurs when an environmental contingency links responses and outcomes." I should have said that it occurs when some variation occurs in the behavior of the organism, quite possibly in a stable environment. Evolution may be accelerated by environmental changes, but the essence of evolution is variation and reproduction in whatever environment presents itself.

Having misunderstood operant conditioning, Timberlake naturally cannot see the parallel with natural selection. He seems to suggest, though here the language is difficult, that nothing in natural selection corresponds to stimulus control in operant conditioning. But if the long neck of the giraffe, to use an outworn example, was selected in terrains in which there was an advantage in being able to eat leaves high on trees, the trait is adaptive only when tall trees are available. Timberlake also suggests that there is nothing in natural selection corresponding to extinction, but I should have supposed that the legs of the whale would qualify as an example.

Timberlake says that I "initially" defined the reinforcer in terms of its effect but "later" argued for a basis in natural selection. I still do both. A susceptibility to reinforcement by a given substance or event is an evolved trait.

Although Timberlake says that a representation of the relation among environment, behavior, and outcome need not be cognitive, he does say that it must be present in some form. "Without the linkage provided by this representation there is no consequence." But all one needs to say is that the organism is changed by the relation; the change need not be a representation of the relation. What is wrong with cognitive science is not dualism but the internalization of initiating causes which lie in the environment and should remain there.

Vaughan accepts the general argument of "Consequences" but adds a useful point about the individual and his place in population studies. The individual is distinguished as such by the variations that have occurred at all three levels, and these are his potential contribution to the future of the species or the culture.

Wyrwicka has misunderstood the parallel I drew between natural selection and operant conditioning. Operant conditioning is an evolved process. Part of it includes an evolved susceptibility to reinforcement by foodstuffs. We, too, have dangerous susceptibilities to reinforcement – for example, by sweets. Until very recently, most sweet things were in short supply but highly nutritious. With the discovery of sugar cane and other sources of sugar, not to mention saccharin, we have constructed a world in which there are altogether too many sweet things to reinforce our behavior. We do not die of starvation; we grow fat.

The effects of operant conditioning are "transferred" only to the same organism at a later date, not, of course, to the species.

I think we can decide whether "animals living in their natural environment . . . care about the taste of food [or] eat only in order to survive." We have only to discover

the taste of food is a reinforcer. I suspect that in
nple organisms eating given foodstuffs is little
m reflex, but if a susceptibility to reinforcement
ved, then redundant support for eating comes
rant conditioning.

nces

D. & Dickinson, A. (1981) Actions and habits: Variations in
tive representations during instrumental learning. In: *Information*
sing in animals: Memory mechanisms, ed. N. E. Spear & R. R.
Erlbaum [WKH]

R. (1952) *Design for a brain*. Wiley & Sons. [TJG]

introduction to cybernetics. Chapman and Hall. [SCS]

G. P. (1941) Fortpflanzungsverhalten und Orientierung der
espe *Ammophila campestris* Jur. *Tijdschrift Entomologie* 84:68–
[GWB]

M. (1896). A new factor in evolution. *American Naturalist* 30:441–
JS]

ental development in the child and race. Macmillan. [TJG]

. (1963). The role of somatic change in evolution. *Evolution*
9–69 [JS]

(1980) Optimal outbreeding and the development of sexual
ences in Japanese quail. *Zeitschrift für Tierpsychologie* 53:231–
[GWB]

R. (1978) Social inhibition of maturation in natural populations of
phorus viriatus (Pisces: Poeciliidae). *Science* 201:933–35. [GWB]

& Richerson, P. J. (1980) Sociobiology, culture and economic
Journal of Economic Behavior and Organization 1:97–
[TJG]

& Breland, M. (1961) The misbehavior of organisms. *American*
ologist 16:661–64. [WKH, rBFS]

D. T. (1956) Perception as substitute trial and error. *Psychological*
w 63:330–42. [RCB, TJG]

Blind variation and selective retention in creative thought as in other
ledge processes. *Psychological Review* 67:380–400. [WKH, rBFS]

Downward causation in hierarchically organized biological systems.
tudies in the philosophy of biology, ed. F. J. Ayala & T.
hansky. Macmillan. [HCP]

Evolutionary epistemology. In: *The philosophy of Karl Popper*, ed.
Schilpp. Open Court Publishing Co. [TJG]

In the conflicts between biological and social evolution and between
nology and moral tradition. *American Psychologist* 30:1103–
[GWB]

H. (1983) Phenotypic plasticity in life-history traits: Demographic
ts and evolutionary consequences. *American Zoologist* 23:35–
[SCS]

orza, L. & Feldman, M. W. (1973) Models for cultural inheritance.
retical Population Biology 4:42–55. [TJG]

Cultural transmission and evolution: A quantitative approach.
eton University Press. [TJG, SCS]

, N. (1959) A review of Skinner's *Verbal Behavior*. *Language* 35:26–
[BD, JWD]

Cartesian linguistics. Harper & Row. [JWD]

Discussion of Putnam's comments. In: *Language and learning: The*
te between Jean Piaget and Noam Chomsky. Harvard University
. [JWD]

Rules and representations. *Behavioral and Brain Sciences* 3:1–
[JWD]

T. (1975) Is a cultural ethology possible? *Human Ecology* 3:161–
[RD]

R. K. (1981) Group selection is implicated in the evolution of female-
d sex ratios. *Nature* 290:401–4. [SCS]

F., ed. (1888) *The life and letters of Charles Darwin*, vol. 3.
ay. [JWD]

R. (1976) *The selfish gene*. Oxford University Press. [BD, SCS]

Replicator selection and the extended phenotype. *Zeitschrift für*
ychologie 47:61–76. [HCP]

The extended phenotype. W. H. Freeman. [RD]

D. C. (1978a) *Brainstorms*. Bradford Books. [AR]

Skinner skinned. In: *Brainstorms*. Bradford Books. [BD]

Why the law of effect will not go away. In: *Brainstorms*. Bradford
s. [BD, JS]

Intentional systems in cognitive ethology: The "Panglossian
digm" defended. *Behavioral and Brain Sciences* 6:343–55. [AR]

Donahoe, J. W., Crowley, M. A., Millard, W. J. & Stickney, K. A. (1982) A
unified principle of reinforcement. In: *Quantitative analyses of behavior*,
vol. 2 *Matching and maximizing accounts*, ed. M. L. Commons, R. J.
Herrnstein & H. Rachlin. Ballinger. [JWD]

Eldredge, N. & Gould, S. J. (1972) Punctuated equilibria: An alternative to
phyletic gradualism. In: *Models in paleobiology*, ed. T. J. M. Schopf.
Freeman, Cooper & Co. [CBGC]

Ellegård, A. (1958) *Darwin and the general reader*. Götеborgs Universitets
Årsskrift. [JWD]

Felsenstein, J. (1974) The evolutionary advantage of sexual recombination.
Genetics 78:737–56. [SCS]

Ferster, C. B. & Skinner, B. F. (1975) *Schedules of reinforcement*. Appleton-
Century-Crofts. [rBFS]

Garcia, J., McGowan, B. K., & Green, K. F. (1972) Biological constraints on
conditioning. In: *Classical conditioning*, vol. 2, ed. A. H. Black & W. H.
Prokasy. Appleton-Century-Crofts. [PRS]

Ghiselin, M. T. (1973) Darwin and evolutionary psychology. *Science* 179:964–
68. [TJG]

(1980) Natural kinds and literary accomplishments. *Michigan Quarterly*
Review 29:73–88. [MTG]

(1981) Categories, life, and thinking. *Behavioral and Brain Sciences* 4:269–
83. [TJG, MTG, AR, JS]

(1982) On the mechanisms of cultural evolution, and the evolution of
language and the common law. *Behavioral and Brain Sciences*
5:11. [MTG]

Gould, S. J. (1977) *Ontogeny and phylogeny*. Harvard University Press.
[WW]

Gould, S. J. & Lewontin, R. C. (1979) The spandrels of San Marco and the
panglossian paradigm: A critique of the adaptationist programme.
Proceedings of the Royal Society of London B 205:581–98. [BD, AR]

Green, S. & Marler, P. (1979) The analysis of animal communication. In:
Handbook of behavioral neurobiology, vol. 3, *Social behavior and*
communication, ed. P. Marler & J. G. Vandenbergh. Plenum
Press. [GWB]

Grene, M. (1959) Two evolutionary theories. *British Journal for the*
Philosophy of Science 9:110–27, 185–93. [BD]

Hamilton, W. (1964) The genetical theory of social behavior. *Journal of*
Theoretical Biology 7:1–52. [JWD]

(1967) Extraordinary sex ratios. *Science* 156:477–88. [SCS]

(1972) Altruism and related phenomena, mainly in social insects. *Annual*
Review of Ecology and Systematics 3:193–232. [SCS]

(1980) Sex versus non-sex versus parasite. *Oikos* 35:282–90. [SCS]

Hanson, S. J. & Timberlake, W. (1983) Regulation during challenge: A
general model of learned performance under schedule constraint.
Psychological Review 90:261–82. [WT]

Harris, M. (1964) *The nature of cultural things*. Random House. [MH]

(1979) *Cultural materialism: The struggle for a science of culture*. Random
House. [MH]

Helmholtz, H. von (1852) On the theory of compound colors. *Philosophical*
Magazine 4:519–34. [PRS]

Honig, W. K. (1981) Working memory and the temporal map. In: *Informat*
processing in animals: Memory mechanisms, ed. N. E. Spear & R. R.
Miller. Erlbaum. [WKH]

Hull, D. L. (1972) Darwinism and historiography. In: *The comparative*
reception of Darwinism, ed. T. F. Glick. University of Texas
Press. [JWD]

(1980) Individuality and selection. *Annual Review of Ecology and*
Systematics 11:311–32. [HCP]

Hutchison, V. H. & Maness, J. D. (1979) The role of behavior in temperature
acclimation and tolerance in ectotherms. *American Zoologist* 19:367–
84. [GWB]

Huxley, J. S. (1942) *Evolution: The modern synthesis*. Allen and
Unwin. [SCS]

Jacobs, H. L. & Sharma, K. N. (1969) Taste versus calories: Sensory and
metabolic signals in the control of food intake. *Annals of the New York*
Academy of Sciences 157:1084–1125. [WW]

James, W. (1890) *The principles of psychology*. Henry Holt and
Company. [JS]

Jenkins, H. M. & Moore, B. R. (1973) The form of the autoshaped response
with food and water reinforcers. *Journal of the Experimental Analysis of*
Behavior 20:163–81. [WKH, rBFS]

Jepsen, G. L., Simpson G. G. & Mayr, E. (1949) *Genetics, paleontology, and*
evolution. Princeton University Press. [CBGC]

Kamil, A. C. & Sargent, T. D. (1981) *Foraging behavior: Ecological,*
ethological, and psychological approaches. Garland Press. [SCS]

Kamin, L. J. (1969) Predictability, surprise, attention, and conditioning. In
Punishment and aversive behavior, ed. B. A. Campbell & Church, R. M.
Appleton-Century-Crofts. [WKH]

Kandel, E. R. (1976) *Cellular basis of behavior*. W. H. Freeman. [PRS]

Kandel, E. R. & Schwartz, J. (1982) Molecular biology of learning: Modulation of transmitter release. *Science* 218:433–42. [AR]

Katz, J. L. & Bever, T. G. (1976) The fall and rise of empiricism. In: *An integrated theory of linguistic ability*, ed. T. G. Bever, J. L. Katz & D. T. Langendoen. Crowell. [JWD]

Katz, M. J. (1983) Ontophyletics: Studying evolution beyond the genome. *Perspectives in Biology and Medicine* 26: 323–33. [MJK]

Katz, M. J. & Grenander, U. (1982) Developmental matching and the numerical matching hypothesis for neuronal cell death. *Journal of Theoretical Biology* 98:501–17. [MJK]

Keller, F. S. & Schoenfeld, W. N. (1950) *Principles of psychology: A systematic text in the science of behavior*. Irvington. [MH]

Kettlewell, H. B. (1961) The phenomenon of industrial melanism in the Lepidoptera. *Annual Review of Entomology* 6:245–62. [SCS]

Lenneberg, E. H. & Lenneberg, E. (1975) *Foundations of language development*, vols. 1 & 2. Academic Press. [GWB]

Lewontin, R. C. (1970) The units of selection. *Annual Review of Ecology and Systematics* 1:1–18. [SCS]

(1982) Organism and environment. In: *Learning, development and culture. Essays in evolutionary epistemology*, ed. H. C. Plotkin. Wiley & Sons. [HCP]

Lorenz, K. Z. (1966) *Evolution and modification of behavior*. Methuen. [RD]

McFarland, D. J. (1966) On the causal and functional significance of displacement activities. *Zeitschrift für Tierpsychologie* 23:217–35. [RD]

Maynard Smith, J. (1964) Group selection and kin selection: A rejoinder. *Nature* 201:1145–47. [SCS]

(1978) *The evolution of sex*. Cambridge University Press. [SCS]

Mayr, E. (1961) Cause and effect in biology. *Science* 134:1501–6 [HCP]

(1976a) *Evolution and the diversity of life*. Harvard University Press. [WV]

(1976b) Typological versus population thinking. In: *Evolution and the diversity of life*, ed. E. Mayr. Harvard University Press. [JWD]

(1982) *The growth of biological thought: Diversity, evolution, and inheritance*. Harvard University Press. [BD, JWD, HCP]

Müller, M. (1872) Max Müller on Darwin's philosophy of language. *Nature* 1:145. [JWD]

Paley, W. (1836) *Natural theology*. Charles Knight. [JS]

Pavlov, I. P. (1927) *Conditioned reflexes*. Oxford University Press. [PRS]

Peterson, G. B., Wheeler, R. L. & Trapold, M. A. (1980) Enhancement of pigeons' conditional discrimination performance by expectancies of reinforcement and nonreinforcement. *Animal Learning and Behavior* 8:22–30. [WKH]

Peterson, N. (1960) Control of behavior by presentation of an imprinted stimulus. *Science* 132:1395–96. [taBFS]

Plotkin, H. C. & Odling-Smee, F. J. (1979) Learning, change and evolution. *Advances in the Study of Behaviour* 10:1–41. [HCP]

1981) A multiple-level model of evolution and its implications for sociobiology. *Behavioral and Brain Sciences* 4:225–68. [TJG, HCP]

Price, G. R. (1972) Extension of covariance selection mathematics. *Annals of Human Genetics* 35:485–90. [SCS]

Pringle, J. W. S. (1951) On the parallel between learning and evolution. *Behaviour* 3:174–215. [GWB]

Pulliam, H. R. & Dunford, C. (1980) *Programmed to learn*. Columbia University Press. [TJG]

Rescorla, R. A. & Wagner, A. R. (1972) A theory of Pavlovian conditioning: Variations in the effectiveness of reinforcement and nonreinforcement. In: *Classical conditioning*, vol. 2. *Current research and theory*, ed. A. H. Black & W. F. Prokasy. Prentice-Hall. [JWD]

Rice, W. R. (1983) Parent–offspring pathogen transmission: A selective agent promoting sexual reproduction. *American Naturalist* 121:187–203 [SCS]

Rosenberg, A. (1983) Fitness. *Journal of Philosophy* 80:457–73. [AR]

Routtenberg, A. & Lindy, J. (1965) Effects of the availability of rewarding septal and hypothalamic stimulation on bar pressing for food under conditions of deprivation. *Journal of Comparative and Physiological Psychology* 60:158–61. [WW]

Schull, J. (in preparation) Evolution, learning, and intelligence. [JS]

Sherman, P. W. (1977) Nepotism and the evolution of alarm calls. *Science* 197:1246–53. [SCS]

Sherrington, C. S. (1947) *The integrative action of the nervous system*. 2d ed. Yale University Press. [PRS]

Simon, H. A. (1966) Thinking by computers. In: *Mind and cosmos*, ed. R. C. Colodny. University of Pittsburgh Press. [TJG]

Skinner, B. F. (1931) The concept of the reflex in the description of behavior. *Journal of General Psychology* 5:427–58 [RCB]

(1932) On the rate of formation of a conditioned reflex. *Journal of General Psychology* 7:274–86. [rBFS]

(1937) Two types of conditioned reflex: A reply to Konorski and Miller. *Journal of General Psychology* 16:272–79. [RCB]

(1950) Are theories of learning necessary? *Psychological Review* 57:193–216. [RCB]

(1953) *Science and human behavior*. Macmillan. [rBFS]

(1957) *Verbal behavior*. Appleton. [taBFS]

(1959) A case history in the scientific method. In: *Psychology: A study of science*, ed. S. Koch. McGraw-Hill. [PRS]

(1964) Behaviorism at fifty. In: *Behaviorism and phenomenology*, ed. T. W. Wann. University of Chicago Press. [AR]

(1969) The phylogeny and ontogeny of behavior. In: *Contingencies of reinforcement: A theoretical analysis*. Appleton-Century-Crofts. [JS]

(1971) *Beyond freedom and dignity*. Knopf. [BD, taBFS]

(1974) *About behaviorism*. Alfred A. Knopf. [CBGC, PRS]

(1975) The shaping of phylogenic behavior. *Journal of the Experimental Analysis of Behavior* 24:117–20. [tarBFS]

(1983a) Can the experimental analysis of behavior rescue psychology? *Behavior Analyst* 6:9–17. [rBFS]

(1983b) *A matter of consequences*. Knopf. [rBFS]

Stearns, S. C. (1983) The evolution of life-history traits in mosquitofish since their introduction to Hawaii in 1905: Rates of evolution, heritabilities, and developmental plasticity. *American Zoologist* 23:65–75. [SCS]

Stearns, S. C. & Crandall, R. E. (1983) Plasticity for age and size at sexual maturity: A life-history response to unavoidable stress. In: *Proceedings of the international symposium of the British Fisheries Society*, ed. R. J. Wootton. Academic Press. [SCS]

Swanson, L. W., Teyler, T. J. & Thompson, R. F. (1982) Hippocampal long term potentiation: Mechanisms and implications for memory. *Neurosciences Research Program Bulletin* 5. [PRS]

Thompson, R. F. (1976) The search for the engram. *American Psychologist* 31:209–27. [PRS]

Thorndike, E. L. (1911) *Animal intelligence: Experimental studies*. Macmillan. [JS]

Thurstone, L. L. (1924) *The nature of intelligence*. Harcourt Brace. [TJG]

Tolman, E. C. (1926) A behavioristic study of ideas. *Psychological Review* 33:352–69. [TJG]

Trivers, R. (1971) The evolution of reciprocal altruism. *Quarterly Review of Biology* 46:35–57. [JWD]

Valenstein, E. S. (1967) Selection of nutritive and nonnutritive solutions under different conditions of need. *Journal of Comparative and Physiological Psychology* 63:429–33. [WW]

Waddington, C. H. (1968) The human evolutionary system. In: *Darwinism and the study of society*, ed. M. Banton. Quadrangle Books. [TJG]

(1969) Paradigm for an evolutionary process. In: *Towards a theoretical biology*, vol. 2, ed. C. H. Waddington. Edinburgh University Press. [HCP]

Wade, M. J. (1978) A critical review of the models of group selection. *Quarterly Review of Biology* 53:101–14. [SCS]

Weimer, W. B. (1973) Psycholinguistics and Plato's paradoxes of the Meno. *American Psychologist* 28:15–33. [JWD]

Weismann, A. (1889) *Essays on heredity and kindred biological problems*. Oxford University Press. [JMS]

Williams, G. C. (1966) *Adaptation and natural selection*. Princeton University Press. [RD, SCS]

Wilson, D. S. (1975) A theory of group selection. *Proceedings of the National Academy of Sciences* 72:143–46. [GWB]

(1980) *The natural selection of populations and communities*. Benjamin/Commings. [GWB]

Wilson, E. O. (1975) *Sociobiology: The new synthesis*. Harvard University Press. [GWB, JWD]

Wynne-Edwards, V. C. (1963) Intergroup selection in the evolution of social systems. *Nature* 200:623–26. [JWD]

Wyrwicka, W. (1975) The sensory nature of reward in instrumental behavior. *Pavlovian Journal of Biological Science* 10:23–51. [WW]

(1980) Mechanisms of motivation in avoidance behavior. *Acta Neurobiologiae Experimentalis* 40:371–80. [WW]